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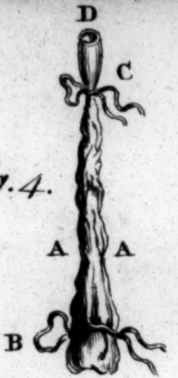

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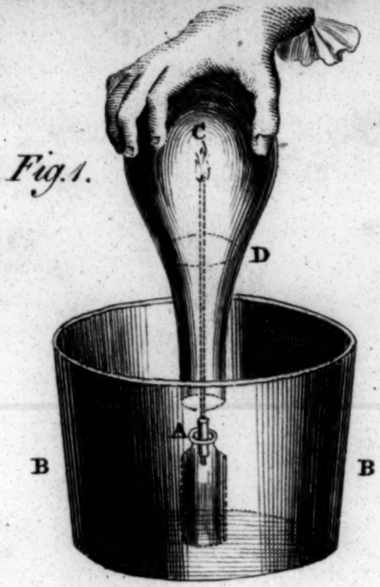

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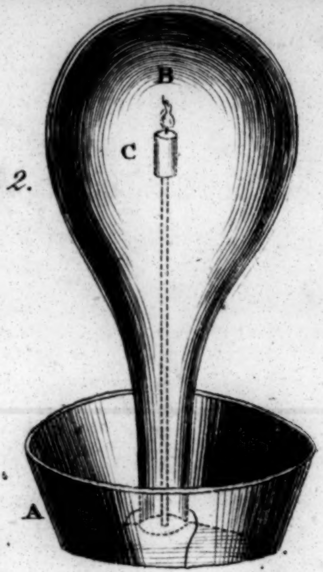

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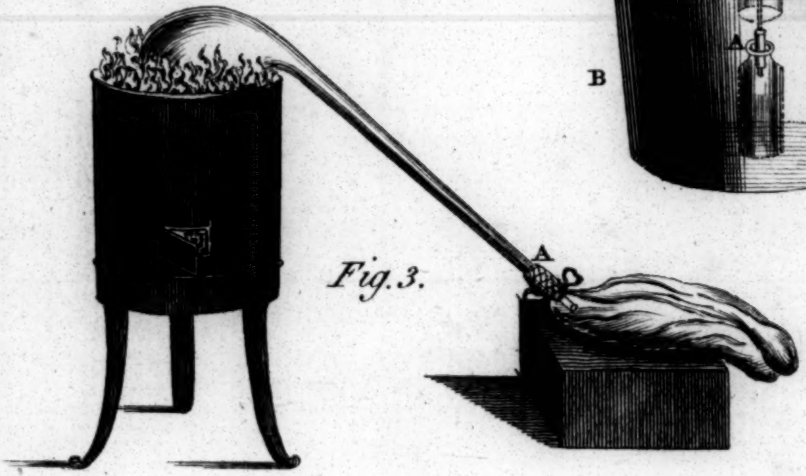

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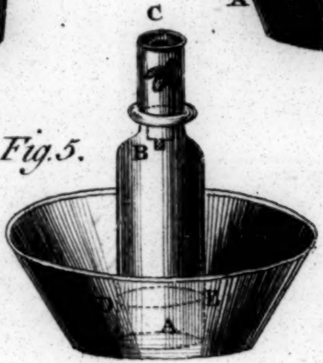

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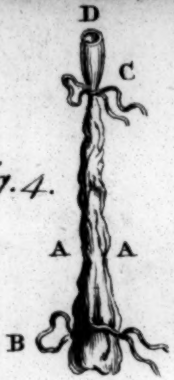
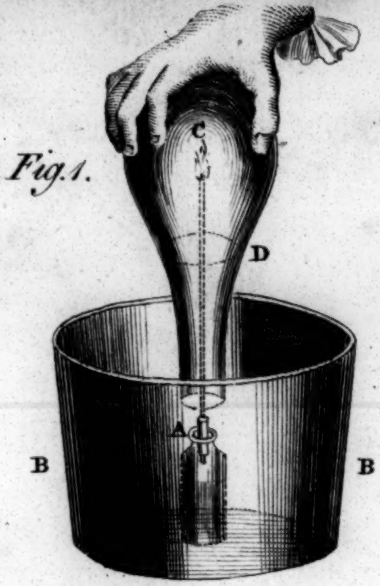


Fig. 1.



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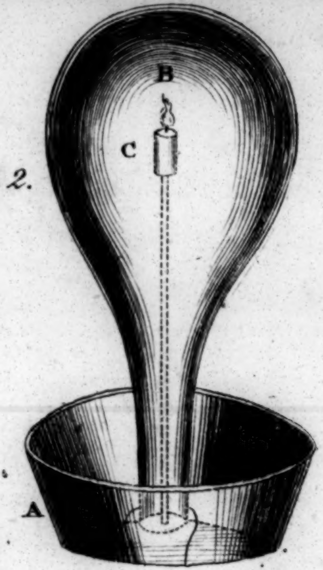

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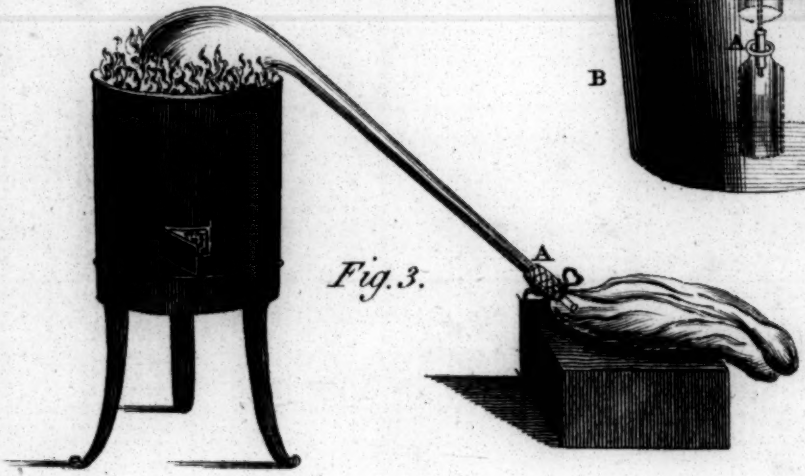

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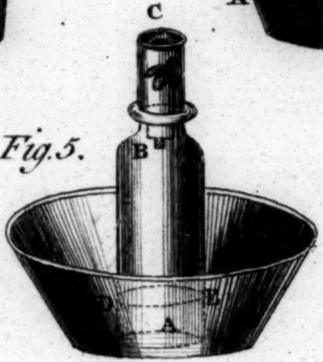

CHEMICAL OBSERVATIONS
AND
EXPERIMENTS
ON
AIR AND FIRE.

BY

CHARLES-WILLIAM SCHEELE,
MEMBER of the ROYAL ACADEMY at STOCKHOLM;

WITH A PREFATORY INTRODUCTION,

By TORBERN BERGMAN;

TRANSLATED FROM THE GERMAN BY

J. R. FORSTER, L.L.D. F.R.S. and S.A.
MEMBER of ſeveral Learned Societies and Academies in EUROPE.

TO WHICH ARE ADDED

NOTES,

BY RICHARD KIRWAN, ESQ. F.R.S.

WITH A LETTER TO HIM FROM

JOSEPH PRIESTLEY, L.L.D. F.R.S.

LONDON:
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TO THE

Rev. Dr. *Joſeph Prieſtley*, F.R.S.

DEAR SIR,

NO ſooner was the preſent Treatiſe of Mr. Scheele, about two years ago, tranſmitted to England from Germany, than you began to encourage me to tranſlate it into Engliſh, and from time to time you repeated your applications. Many avocations of another nature hindered me from anſwering your friendly expectation. Laſt winter you not only renewed your ſolicitations, but by the aſſiſtance of your friends enabled me to complete the work, which I had already begun. It is with heartfelt pleaſure, and real gratitude and reſpect for your friendly encouragement, that I now take the liberty to preſent you with the tranſlation of a work on Air and Fire, which has in many reſpects ſo great a reference to your immortal diſcoveries on Air. The northern philoſopher has treated the ſubject as a Chemiſt, you as a Philoſopher; he came in many reſpects to the ſame concluſions, grounded however on different premiſes; you knew nothing of his Experiments, and he was ignorant of your great and numerous diſcoveries, when he made his Experiments: and you both, independent of one another, contributed by very different ways and methods to enlarge the field of ſcience. The differences in our Author's performance, ariſe chiefly from his not attending always to ſome principles of natural Philoſophy, or elſe from adhering too ſcrupulouſly to chemiſtry, and not ſo much founding certain inferences on Experiment as on reaſoning.

Though invited both by you and others to add ſome notes to Mr. Scheele's perforformance, I made uſe of this privilege of Tranſlator and Editor very ſparingly; and only in thoſe caſes, where I thought the ſenſe wanted illuſtration, or where I was afraid the Author might be unintelligible or obſcure, without a ſhort remark, explaining perhaps ſome chemical compound, or a cuſtom uſual in Sweden and the North only and therefore little known here.

Give me leave to add a few words relative to my tranſlation. I have endeavoured to make it as literal as was conſiſtent with the genius of both languages. The ſtyle of my Author is not always ſo ſmooth and clear, as might be wiſhed in a ſcientific performance: wherever the German could be plainly underſtood, but might in the Engliſh cauſe obſcurity or ambiguity, I did not heſitate to inſert an explicative word or ſentence, in order to make the ſenſe run ſmoother and clearer. Where the original carried with it obſcurity or ambiguity, I endeavoured to be as literal as poſſible, and neither to add nor to take away any part of the ſentence. A book of ſo great importance in ſcience, ſhould not be tranſlated in a careleſs manner. This very importance gives me the beſt grounded hopes, that the remarks and ſtrictures which you or your friends intend to annex to it, will ſerve to elucidate many a point, which our Author could not know, or perhaps conſidered in a different light.

May Providence ſpare your life, and bleſs it with health, contentment of mind, and all thoſe comforts and enjoyments which may more and more enable you to purſue that noble career of Natural Philoſophy, which you have already ſo much enriched by your numerous diſcoveries, and which you would moſt certainly ſtill more aſcertain and enlarge by new and intereſting Experiments.

I have the honour to be, with the ſincereſt ſentiments of attachment, gratitude, and regard,

Dear Sir,

Your moſt obedient,

Obliged humble Servant,

Paddington-Green,
Feb. 28, 1780.

John-Reinold Forſter.

THE AUTHOR's PREFACE.

THE reſearches on *Air* are at preſent an important object of Chemiſtry. This elaſtic fluid is poſſeſſed of ſo many properties, that it may furniſh ſufficient materials for new diſcoveries to every one who chuſes to ſet about making Experiments. This branch of Chemiſtry, ſhews us, that *Fire*, that is ſo wonderful, cannot be generated without Air: I hope not to be looked upon as erroneous, when I preſume in this *Treatiſe*, (which ought to be conſidered only as an Eſſay towards the Chemical Doctrine of Fire) to alledge proofs, that a kind of Air ſubſiſting in our atmoſphere is a true conſtituent part of Fire, and materially contributes to the exiſtence and ſupport of flame; for which reaſon I call this Air *empyreal Air* (Fire Air). Certainly I will not be ſo impertinent as to force my opinion upon my Readers: No; there are evident Experiments that ſpeak for me; Experiments which I have many times repeated, and in which I flatter myſelf to have attained my view of coming as much as poſſible at the knowledge of Fire: and this is all the reward I have obtained for my labours, together with the real pleaſure I then enjoyed, which I was unable to keep to myſelf. This therefore is the view in which I have communicated my labours to my Readers.

I had already elaborated the chief part of my Eſſay, when I firſt got a ſight of the excellent Experiments of Mr. PRIESTLEY. Though the theory of Meyer has met very little approbation, his Experiments however will always have merit; and I am ſure this laborious Chemiſt would have changed his opinion, could he have had the leaſt notion of fixed Air, or the theory of Dr. Black: But ſince in the time when he publiſhed his Book *, this doctrine was not yet ſo well examined, it is no wonder that he ſtill adhered to the old opinion that all pure alkalies and abſorbent earths muſt effervefce with acids. That I have myſelf gone too far in this Treatiſe with my chemical Experiments, will perhaps be made matter of objection: but I am of opinion, that only in the caſe, when no more material ſubjects are left for objects of enquiry, ſhould this ſcience be defined by limits. But who will pretend to prove that light and heat do not belong to the corporeal ſyſtem? for though they be very ſubtle and fine, it will be eaſy to prove ſo far from my Experiments, that they are compounds of two different ſubſtances, (conſequently can no more than Air, be conſidered as primitive Elements) and that by thoſe incomprehenſible operations of nature, the attractions, they are very often decompounded into their integrant parts: for did this

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* *Joh. Fred. Meyer's* Chemiſche Verſuche zur nahern Erkentniſs des ungeloſchten kalchs. Hanov. & Leipz. 1764. The ſame has been lately tranſlated into French.

not take place, it would be right to put the queſtion, Whence all the great quantity of Empyreal Air is generated; ſince every moment ſuch quantities of empyreal Air are deſtroyed by animals, or changed into aërial acid, and employed for the compoſition of Fire?

Whether I am wrong or right in my opinion on the *conſtituent parts of earthy ſubſtances*, time muſt decide: ſo much however I believe, might be ſaid with juſtice, that my opinion is ſomewhat more than a mere hypotheſis, ſince it is founded on Experiments.—I take it as a certainty for granted, that *pure water in itſelf, can neither by art nor nature be changed into a dry ſubſtance, which ſhall have all the properties of a true earth.* I am not ignorant of the circumſtance that both by repeated diſtillations, and likewiſe by trituration of pure water, an earth has been produced. It was notſufficient for me merely to read this very ſtrange metamorphoſis, I wanted likewiſe to behold it with my eyes: I took a quarter of an ounce of diſtilled ſnow-water, put it into a ſmall glaſs matras of the ſhape and ſize of a hen's egg, with a ſlender neck of two feet length; I cauſed the water to be boiled in it, and then ſhut the orifice to it up with a well-fitting cork: I then ſuſpended this matras over a burning lamp, and kept the water continually boiling for twelve days and nights together: after it had boiled two days, it had ſome whitiſh appearance; after ſix days time, it was white like milk; and at the end of the twelfth day, it ſeemed to be quite thick. After it was grown cold, I let the matras ſtand peaceably, that the white powder might ſubſide, which happened only in two days time; then I decanted the *water* quite clear, which I found to have the following properties: Mixed with ſalt ammoniac, it ſets the volatile alkali free: It coagulated by pouring on it acid of vitriol; it precipitated metallic ſolutions; it ſtruck the ſyrup of violets with a green colour; and in the open Air it turned into a jelly. The very ſubtle and white *earth* had the properties of ſiliceous earth mixed with a little of calcareous ſubſtance. I broke the matras, and found the inſide of it dim, and without the ſmoothneſs common to glaſs, as far as the boiling water had reached; though this only became viſible when the glaſs was dry. Could I doubt any longer, that the water, by continued boiling, is capable of decompounding the glaſs? Had I not obtained a true *liquor ſilicum?* The earth which I procured was therefore by no means generated from water.---No better ſucceſs I met with, when I triturated diſtilled water during two hours in a poliſhed glaſs mortar: The water became milky; after the white ſubſtance was ſettled, I decanted the water; which had all the properties of pure water, without any addition of alkali; and the white earth was nothing elſe than the moſt ſubtle powder of glaſs.

I have nothing to add, except that ſeveral typographical *Errata* have crept into this Treatiſe, which are the more excuſable ſince I live very far off from the place of impreſſion; but they are all of ſuch a nature, that neither the main ſubject nor the ſenſe is affected by it.

NOTE *of the* TRANSLATOR.

JOHN MAYOW, in his Opera Omnia Medico-phyſica, Hagæ, 1681, 8vo. has already given ſome obſcure hints about that part of our common atmoſpherical Air, which properly ſpeaking is the moſt proper for reſpiration, or in which the flame of a candle will burn longer than in the ſame bulk of our mixed common Air. This conſtituent part of our common Air Dr. PRIESTLEY has called *dephlogiſticated Air*, and Mr. SCHEELE *empyreal Air*; it is eaſily produced by heating red lead, moiſtened with ſpirit of nitre; and Dr. Prieſtley obſerves, that dephlogiſticated Air is a compound of ſpirit of nitre and earth. (See *Experiments and Obſervations on Air*, vol. ii. p. 54, 55, 62, 63. vol. iii. p. 41.) This above-mentioned *John Mayow* pretends, that ſome ſalt-petre, or even aërial ſpirit of nitre flies about in the Air; which when breathed enters the lungs and yields in the human body the animal ſpirits and heat which it communicates to the maſs of the blood. By reſpiration the Air loſes that elaſticity which it had obtained from that kind of nitre which he ſuppoſes in common Air.— This dephlogiſticated Air, however, though very obſcurely, ſeems to be hinted at in *Mayow's Diſſertatione de Salnitro & Spiritu Nitri Aëreo*; item in the *Diſſ. de Reſpiratione*, contained in the above *Opera*. The honour of the diſcovery of this kind of Air is ſo much the property of Dr. Prieſtley and Mr. Scheele, though neither knew of the other's Experiments, that there is not the leaſt doubt about it: But is it leſs certain, that *Mayow* proved his theory in a manner ſo little convincing by Experiments, that he cannot havē the leaſt claim to any diſcovery? It is only curious, that a looſe hint, thrown out at random in that age, ſhould at ſuch a diſtance of time in a new, more circumſtantial and ample manner be confirmed in a ſeries of Experiments by two Philoſophers who certainly knew nothing of Mayow.

PREFATORY

INTRODUCTION;

Written by Sir TORBERN BERGMAN,
(Knight of the Order of Waſa, Profeſſor of Chemiſtry in the Univerſity of Upſal, F.R.S. and Member of the Academies of Sciences at Upſal, Stockholm, Goettingen, Berlin, Gottenburg, and Lund, in Scania.)

THE Science of Nature ſeems to have three degrees. The firſt fixes our attention to the *outſides*, and teaches us to collect external characters, in order to enable us to diſtinguiſh various natural bodies; and this is the proper object of NATURAL HISTORY. If we penetrate ſtill deeper by our contemplation, and examine the *general qualities* of matter (its extenſion, impenetrability and *vis inertiæ)* in regardto its peculiar relations; it is that which is commonly called NATURAL PHILOSOPHY *(Phyſica)*. But CHEMISTRY is the *innermoſt part*, ſince it examines the material elements, their mixtures, and proportions to one another. The firſt teaches us the elementary rudiments, the alphabet of the great book of nature; the ſecond inſtructs in ſpelling; and the third, to read diſtinctly. The two firſt therefore are no more than ſubſidiary ſciences, which conduct us to the laſt, as the proper great object.

Since all ſingle qualities of bodies depend upon their ſtructure and compoſition; it follows neceſſarily, that all our occupations with material and corporeal ſubſtances, never can obtain any degree of perfection, without the aſſiſtance of Natural Philoſophy. And ſince commonly the proper intention of our occupations is directed either to our health, the neceſſaries of life, or our comfort, three different claſſes of practical ſciences are from thence derived, Medicine, Huſbandry, and the Arts. We ſhall in the moſt conciſe manner conſider the connection of chemiſtry with each of them.

In the times when chemiſtry was chiefly founded on various nonſenſical hypotheſes, and was nevertheleſs applied with a blind zeal and the moſt perfect confidence not only to the cure of all infirmities and diſeaſes, but even to the obtaining of immortality itſelf; then it could be productive of nothing but detriment and miſchief in the ſcience of phyſic.

We have no knowledge of bodies *a priori:* Every intelligence about them, muſt be acquired by proper obſervations and Experiments. But to diſcover and purſue ſuch Experiments as really illuſtrate the point we are in ſearch of, requires not only ſkill, and a peculiar application, but alſo the moſt impartial love of truth; in order not to be enſnared by the pleaſing deſire of drawing general concluſions from a few *data* of precarious certainty. It leſſens no doubt our trouble, and flatters our vanity, to be able to diſcloſe in a moment the whole courſe of nature. Man is beſides naturally indolent, and much inclined to be captivated by imagination more than by reality. The confeſſion therefore, that we really know no more than what we know, is even in our days, when the experimental method is conſidered as the only right and true method, very difficult and humiliating: But ſince all ſciences have their roads of error, their abuſes and follies (which unfortunately always ſeduce the greater part of mankind) it is chiefly owing to this precipitation and fancy, that ſuch monſters are produced.

Every thing going forward in our body, is done either in a *mechanical* or in a *chemical* way; ſince the operations of the immaterial ſoul have nothing to do with it. Our food is diſſolved and changed on its road in paſſing from the mouth, through the ſtomach, entrails and other inteſtines, by means of the *ſaliva*, the *liquor gaſtricus*, the gall, the chyle, &c. ſo that in ſeveral places various matters are prepared, all of which are neceſſary for the increaſe and ſupport of the machine! the lungs moreover add by their conſtant motion, various ſubtle particles by means of the abſorbing veſſels, and again by the *exhaling veſſels*, carry off others. The leaſt irregularity in all theſe natural functions, lays of courſe the foundation for many infirmities and diſeaſes. Theſe ought to be prevented or cured by ſuch medicines as have nothing noxious in other reſpects capable of obſtructing the main tendency, and which contain nothing ſuperfluous; for if they were even in themſelves harmleſs, they might however become hurtful by their quantity. It is therefore no doubt a great advantage, to be able to concentrate the powers of ſeveral ounces into a few grains.

Did we exactly know after chemical principles, the natural functions going forward in the body, the cauſes of diſeaſes, and the effects of medicines; chemiſtry would moſt certainly operate miracles.

But I foreſee already what may be objected to this aſſertion with ſome reaſon, viz. "Though ſuch knowledge would be a "moſt excellent thing, it would how"ever be rather the object of wiſhes than "ſuch as might be ſuppoſed or hoped "for: In the human body every ope"ration goes forward very ſecretly;

" nor is there any glaſs window to admit " the reſearches of the inquiſitive: It is " by no means right to play with the lives " of men, and it would be highly unjuſtifiable to endanger them by uncertain " Experiments, &c." I reply, this is perfectly right; at the ſame time it by no means juſtifies us in neglecting the right means of obtaining information; and ſtill leſs in treating them with contempt. Whatſoever is difficult, is not therefore impoſſible always. The more inveſtigation and trouble an invention coſts, the more honour may be obtained by it; eſpecially if it has for its object, Health, the moſt precious enjoyment a man can be bleſſed with. If a man be only poſſeſſed of a truly fundamental knowledge, many things may be diſcovered without the leaſt danger. Higher chemiſtry has diſcovered many and various new manipulations, and by recalling to our minds one or the other inſtance, we ſhall be enabled to judge what more may with reaſon be expected.

Several kinds of diſeaſes ſpread devaſtation over extenſive countries, over people of all ranks and denominations; without being however infectious in themſelves. Theſe cannot, properly ſpeaking, be derived from the mode of living; which in thoſe who are attacked with the evil, is widely different; there muſt therefore exiſt a general cauſe, which affects equally the richer and the poorer. The *atmoſphere* is the ſame for all; during ſeveral years its weight and temperature have been obſerved; and this has furniſhed ſeveral explications, which however are not ſatisfactory for the explanation of all its phænomena. Its conſtituent parts therefore ought to be better known. The vapours and heterogeneous particles, are different in regard to quantity, as well as to quality. Beſides Chemiſtry teaches us, that this elaſtic fluid ſurrounding our globe, is at all times and every where, a compound of three very different ſubſtances, *viz.* of good Air, foul Air, and aërial acid. The *firſt* is called by Dr. PRIESTLEY, if not wrongly, however ſomewhat improperly, *phlogiſticated Air.* *

Mr.

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* Though it is expreſſed here plainly in the German, *phlogiſticated Air*; I am however pretty well convinced, that either Chevalier Bergman perfectly

miſtook

Mr. *Scheele* calls it with more reaſon *empyreal Air*, (Fire Air) ſince this alone gives life to Fire, when the other two kinds of Air extinguiſh the flame of a candle or Fire. The laſt kind is commonly called *fixed Air*: but I flatter myſelf to have ſatisfactorily proved by Experiments, that it is a peculiar acid. The nature of the *firſt* * kind, has as yet been very little inveſtigated; however by analogy it ſeems to be hardly any thing but good Air, which is corrupted either by a ſuperfluity of inflammable ſubſtance, or perhaps by a want of

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miſtook Dr. Prieſtley, who calls that *dephlogiſticated Air* (and not *phlogiſticated*) which is ſo good that Fire will burn in it longer than in any other kind of Air; or there muſt be an error in the preſs, and we ought to read *dephlogiſticated* inſtead of *phlogiſticated* Air; and indeed that great Philoſopher and Chemiſt Chev. Bergman could hardly be ſuppoſed to be ſo ignorant, as not to know the difference between *dephlogiſticated* and *phlogiſticated Air*.

* In the German it ſtands as here, *firſt kind*; but from the connection, it ſeems, Mr. Bergman meant the *ſecond kind* of the three different ſubſtances which he obſerved to be the integrant parts of common atmoſpheric Air, or (as he calls it) *foul Air*.

of it. It will however be ſtill difficult to decide which of the two above propoſitions is the true one.—Of theſe three diviſions, the aërial acid always bears the leaſt proportion, and ſcarcely makes the fifteenth part of the bulk of the atmoſphere, at leaſt on the ſurface of our globe: the foul Air is always in the greateſt proportion; and by far more, than the pure or good Air.

The various effects of each of theſe three ſubſtances upon animal bodies, are ſtill involved in impenetrable darkneſs.—The good Air fit for reſpiration, ought however to be excellent in its operations, ſince without it, it is impoſſible to live. It has been believed, that it contained an indiſpenſibly neceſſary vivifying food; which never has been proved, at leaſt it ſeems not to be of an *electric* nature. It might perhaps ſoon be poſſible to decide by Experiments, whether this good Air carries off noxious, eſpecially phlogiſtic particles; ſince the Air which has been expired from the lungs, is unfit for reſpiration, and is ſimilar to that which has been phlogiſticated. —We ſhall then likewiſe be informed, whether that which makes the greater part of the atmoſphere, if it be inſpired by itſelf, becomes fatal, for the reaſon that it has been previouſly ſaturated with heterogeneous bodies, and is thence incapacitated for carrying off any particles from the lungs. —Perhaps the aërial acid becomes an unfit vehicle for theſe articles, and is itſelf originally formed without them; however it is not yet aſcertained in what manner this Air acts; but thus much is certain, that it deſtroys all irritability. I have, from animals killed by it, before they were grown cold extracted the heart, in which it was impoſſible to raiſe the leaſt irritability, either with the moſt powerful *menſtrua*, or the fire, or ſcalpels. If muſcular fibres were the chief cauſe of the motion of the lungs, it would not be difficult to find out the ultimate cauſe of death; but ſince its ſtructure is formed of quite different ſubſtances, the greateſt difficulties preſent themſelves. By Experiments however it might be eaſy to aſcertain, whether the aërial acid and foul Air act in the ſame manner. It would therefore be neceſſary to inveſtigate, whether irritability (by all appearance ſo very neceſſary for the whole œconomy of animal bodies) is not likewiſe deſtroyed in ſuch animals as have been ſuffocated and killed inſtantaneouſly by that Air, which had been corrupted by reſpiration, fire, or other means. Since the Air returning from the lungs, by expiration, is always blended with aërial acid; it would be neceſſary, in order to avoid all ambiguity, entirely to free the foul Air of the aërial acid by means of lime-water, before any Experiments were made with it. I hope ſoon to have an opportunity of making them. If then, contrary to expectation, the reſult ſhews, that they both act in the ſame manner it would ſeemingly follow from thence, that theſe two fluids, ſo very diſtinct in their nature, act however by the ſame common unfitneſs, either for carrying off from the lungs the noxious particles, or introducing into them a vivifying *pabulum*.—But however noxious aërial acid is for ſound lungs, it is nevertheleſs of great utility in the *primæ viæ*. Purulent lungs not only bear aërial acid, but they are even cured by it: And no ſooner is the cure performed, than its inhalation becomes again dangerous.—But though there be ever ſo much left for inveſtigation, we may apply that for uſe which has already been diſcovered. It is for inſtance well known, that the Air of the atmoſphere may be examined in regard to its fitneſs for reſpiration; a diſcovery which bids fair to yield, in a very ſhort time, the moſt important illuſtrations. If ſuch obſervations were made at the ſame time in dwelling-rooms, infirmaries, and the open Air, and were continued with ſome degree of accuracy, we ſhould certainly diſcover the cauſes of many phænomena, which hitherto have remained inexplicable. Thus much we know already; that wounds and ulcers are with difficulty kept from going into mortification in corrupted, foul Air; whereas on the other ſide, the immenſely acute pains accompanying the cancer (that dreadful ailment) are in a few days not only mitigated and aſſwaged by the external uſe of aërial acid, but the loathſome aperture is alſo conſiderably diminiſhed—.The compaſs of a Preface not only prevents me from entering into a fuller diſcuſſion of this important ſubject, but alſo from producing more inſtances of its utility; I cannot help however mentioning in a few words only, that the obſervations on aërial acid have already ſpread a new light on the method of eſſaying and imitating, by art, medicated waters; and on their application and uſe in the cure of the ſcurvy, and other internal putrid diſeaſes.—The true analyſis of the calculi from the kidneys and bladder, has enabled us to judge with preciſion of the beſt remedies againſt the gravel. The diſcovery of the inner conſtitution of arſenic, made us better acquainted with the manner by which its dreadful effects are produced; and thereby the beſt means were pointed out, both to extinguiſh its poiſonous quality, and likewiſe to mitigate and direct its effect for better purpoſes. How ſimplified are at preſent many compounded remedies; and how ſafe and little perilous, are many preparations of the moſt acrid and corroſive ſubſtances? How many abſurd mixtures, decompounding and counteracting themſelves, have not of late been rejected? How many falſe theories of diſeaſes, and their cauſes, are there not daily expoſed, and ſhewn to be without any foundation, by means of Chemiſtry? Sugar was ſaid to contain lime, in order to account for certain effects aſcribed to ſugar; though it contains not an atom of it. The calculi of the kidneys and the bladder were ſaid to be calcareous; though they contain at the moſt only one half of calcareous ſubſtance. And ſo in many more inſtances, which it would be too tedious to enumerate. Laſtly, how would it be poſſible to diſcover and to ſubject to controul the various *noſtrums*, quack-medicines, cheats, neglects, and other monſters of the medical ſcience without the application of a chemical analyſis? The Muſes were not without reaſon thought to be ſiſters; and they are a moſt excellent emblem of that harmonic union, by which all the ſciences mutually aſſiſt one another; and without which they never can attain any degree of perfection.

Next to health, there is nothing more preſſing and neceſſary than *common food*. In order to convince us of the utility produced by Chemiſtry in regard to the neceſſaries of life, we ſhall only take a view of *Agriculture*, that moſt antient and noble occupation. If we conſult *Columella*, and many of the moſt antient writers on Huſbandry, we find to our humiliation and ſhame, that they were as well inſtructed as us, if not better; though in later times ſo much pains have been taken, ſo many encouragements have been given, and ſo many rewards have been diſtributed.—This ought to be conſidered in the following manner. The great Lord of Nature has ſo bountifully taken care of us, that corn grows without any great care and knowledge. The improvements which this art in general is capable of from experience and collected Experiments, are likewiſe ſoon enough acquired; and in this ſtate it remains till the ſcience of nature throws a new light upon it. They are two very diſtinct things, to raiſe corn on a certain piece of ground, and to raiſe as much corn as will poſſibly grow there:—The laſt deſideratum is not always obtained by ploughing, ditching and manuring; for beſides theſe mechanical operations, there are two things more required: ſuch a mixture as will afford to the vegetables not only proper nouriſhment; but alſo ſuch a one as keeps humidity as long as the uſual drought makes it requiſite; for nothing grows, even in the moſt exquiſite ſoil, without water. The beſt compound therefore, ought to be adapted to the nature of the ſoil, the expoſure, the climate, and the common temperature of the weather; as I have proved more at large in another place.—In the mean time it will be eaſily found, that Chemiſtry is of the ſame importance to agriculture and all the more particular branches of rural œconomy, that aſtronomy is to navigation.

The arts and mechanical trades, are occupied with the melioration of raw materials. One part of them, from beginning to end, is a ſeries of chemical operations; others are more mechanical; however there is hardly one of them which contains not one or more problems whoſe more perfect ſolution depends upon Chemiſtry only. What a number of years muſt have elapſed before they arrived by chance only, or by unpremeditated Experiments, to their preſent perfection; or before they learned to avoid all occurring inconveniences. A perfect knowledge of the raw materials, often furniſhes the beſt inſtructions, in a direct way. And it has been hitherto a great misfortune, that the practical part of the Arts has been kept very ſecret: But ſince the French Academy of Sciences has begun to remove this obſtacle, we have the moſt certain expectations of their quick and amazing progreſs, by the aſſiſtance of the ſcience of Nature. Without the knowledge of the true cauſes and their connections, it is impoſſible either to obviate or to remove all the obſtacles ariſing from chance or accidental circumſtances.

From what has been ſaid in a few words, it muſt evidently appear, that Chemiſtry in a peculiar manner, throws a great light on all mechanical trades, concerned with corporeal ſubſtances.—However the property of the ſubject itſelf ſeems to preſcribe limits to this ſcience. The quality of our ſenſes, even when aſſiſted and improved by art, will not let us penetrate farther than to a certain point. The ſubtleneſs of our inſtruments is inſufficient, and the beſt become at laſt uſeleſs. All theſe difficulties are ſtill more increaſed, from the moſt ſubtle particles which cannot well fall under the examination of our bare ſenſes, having not only the greateſt adheſion and conſequently exerciſing the greateſt reſiſtance to the attempts of dividing them, but alſo affording the moſt effectual and active means for this end. How imperfect therefore, muſt be our moſt perfect knowledge of the myſteries of nature?

This is all perfectly right; and the pretenſion is ſupremely ſilly, to be able at a future period to inveſtigate the firſt powers, which the Creator has eſtabliſhed at the creation and for the preſervation of the material world. Such a ſcience is too abſtruſe, and reaches beyond our attainment; it is reſerved for the power of the Creator, and not for that of a creature. — But it by no means follows, that Chemiſtry ſhould not be able to diſcloſe to our eyes many a ſcene of wonders. Could we only diſcover with certainty, the elementary principles of bodies (principia proxima) together with their connections and proportions, Chemiſtry would be able to do great things.

From the moſt early periods, certain ſubſtances were thought to be ſo ſimple, that art gave up all hopes of analyſing them any further, and theſe were therefore conſidered as the *ſtamina prima*. Such are more eſpecially the four Ariſtotelic elements, Earth, Water, Air, and Fire: To diſcover the compoſition of which is the buſineſs of higher and more refined Chemiſtry. Let us therefore examine, whether all hopes are entirely loſt of diſcovering any thing here.—EARTH is the fixed principle, which remains after the Fire has exerted its deſtroying power againſt a body, and which is not ſoluble in water in the common way. This is the coarſeſt of all the four common elements, and makes but a ſmall part of the bodies. Whatever analyſis has hitherto been able to eſtabliſh with certainty, may be reduced to this; that earth (which after all proves to be of different ſubſtances) is by no means the ſame matter, nor homogeneous, but a compound of ſeveral; and is found to be more or leſs of a ſaline nature, at leaſt it is ſo on the ſurface of our globe, in it its moſt pure ſtate. There have been found ſix different ſpecies; having all quite different properties, and which hitherto have neither been decompounded into more ſimple, nor been changed into another: viz. 1. The *Earth of* the *ponderous*

Spar; 2. *calcareous Earth*; 3. *Magneſia*, 4. *Clay*; 5. *ſiliceous Earth*; and 6. *Earth of Gems*. I have in another place mentioned the characteriſtics of them.— Whether theſe, which till a further inveſtigation may be called *primitive Earths*, are really different, or only varieties of the ſame, I cannot tell; the laſt opinion ſeems to be the moſt ſimple. I have however before obſerved the dangerous error of limiting the order of nature to our confined ideas. The concluſion ought not to be formed before the premiſes are aſcertained by Experiments. Patience and aſſiduous application may perhaps in future times make diſcoveries; ſince we have at leaſt ſome conſiderable number of notions about their properties, by which a great deal is already gained.—But ſince the acid of fluor and water, meeting one another in the form of vapours, coagulate into ſiliceous earth; and acid of arſenic with phlogiſton coagulate into ſolid white arſenic; there is ſome indication that terreous ſubſtances, as well as metallic calxes, may be conſidered in their firſt principles as acids; which in the firſt caſe are become fixed by water, and in the laſt by phlogiſton. Thus much at leaſt is certain, that nature has indicated itſelf to be poſſeſſed of an abundance and variety of acids; and that it particularly employs them in its various operations.

Water is ſtill more ſubtle, and with more difficulty decompounded.— Upon more accurate examination we find, that the Experiments according to which it was imagined, that water could be changed into earth, do by no means prove that which in the beginning had been ſuſpected of them. It is generally known, that heat cauſes peculiar changes in water. The particles of water ſtrongly attract the particles of heat; and after they have been charged with, or envelloped by a certain quantity of them, this compounded matter becomes ſo eaſily moveable, that its ſurface endeavours conſtantly to form a horizontal line; it has a great ſimilarity with a ſubtle earth melted by fire. If you diminiſh the heat, by which operation the ſurface of the particles either come in contact and by their friction hinder this reciprocal motion, or the elaſticity and power of repulſion is ſufficiently diminiſhed by the ſeparation of one of theſe elementary principles, then the maſs becomes indurated and ice is formed. It is not yet determined, which of theſe cauſes is here

prevalent,

prevalent. If ice is to be melted, a quantity of the employed heat is loſt, amounting to very near 72° of our thermometers, * and forms a kind of ſaturation; ſo that its effect is concealed by its union with the ice, almoſt in the ſame manner as if an acid were hindered from exerciſing its characteriſtic properties by a ſaturation with alkalies. The ſame takes place with quicklime: It contains heat, which is inactive till a ſtronger elective attraction ſets free and expels it. By the abſorption of 72° of heat, ice is made fluid; that which it receives beyond it is ſuperfluous; which may be eaſily obſerved, in the ſame manner as in adding an acid to a neutral ſalt: The water expands, grows warmer, more ſubtle, more penetrating, moveable, and light. If the quantity of heat is accumulated till it becomes equal to 100°. it is diſſolved into elaſtic vapours; ſome of which are already generated before the degree of heat is equal to 100°; but ſo much the leſs, the leſs the quantity of heat exceeds the abovementioned degree of ſaturation.—— In the very moment they are ſeparated from the maſs it cools; for all evaporation generates cold. Does this cold ariſe becauſe the increaſed volume requires more to its ſaturation, and can in conſequence of its effect, fix more heat than before? Or is the heat, (which was united to the water and was increaſed to a certain degree) now enabled to conſolidate itſelf cloſer and in greater abundance, and to carry off in conſequence that part of the water which is neareſt at hand? ——This is what Experiments have taught us of the compoſition of water; plainly demonſtrating that it by no means ought to be conſidered as a ſimple ſubſtance.

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* The Swediſh thermometers are filled with quickſilver, and the point of congelation of water, is diſtant from that of boiling water exactly 100 parts or degrees. The point of freezing is 0 and that of boiling, is one hundred upwards. From 0 downwards, another ſeries of degrees is marked for the obſervation of cold.

I have before mentioned ſomething about AIR, and proved, that the ſubſtance commonly called by that name, is by no means a ſimple and homogeneous body. I need not likewiſe be prolix on this, and the thus called fourth element or FIRE; ſince theſe two are the objects of the following performance;—whoſe author, Mr. SCHEELE, has great merit in Chemiſtry by ſeveral important diſcoveries: * all his inquiries ſhewing not only his reflection and reaſoning, but alſo a peculiar ſkill and perſeverance in inveſtigating truth, both by analyſis and by ſyntheſis. *Newton's* diſcovery to divide light (the infinitely ſubtle light) into colours, has opened the way to many very curious myſteries of nature, though it was no more than a mechanical diviſion. Mr. *Scheele* diſcloſes a more ſubtle, becauſe a chemical analyſis; which inſtructs us not only about light, but alſo about fire, whoſe explication has hitherto been the *crux philoſophiæ*. I have, with ſeveral alterations, repeated his principal Experiments on which he grounds his doctrine on this ſubject, and found them perfectly accurate. Though in ſome leſs material circumſtances a nearer confirmation might become neceſſary; it does however no injury to the main ſubject; which is grounded on ſeveral correſponding Experiments.—Heat, Fire, and Light are in regard to the elementary principle, the ſame with good Air and phlogiſton; but their proportion and perhaps the manner of their compoſition, cauſe the great difference. Phlogiſton ſeems to be a real elementary principle, which enters the chief part of ſubſtances, and adheres to them moſt obſtinately. There are ſeveral means to ſeparate it more or leſs perfectly: Of thoſe known ſubſtances, good Air is moſt active; for which reaſon, I put its ſign at the very top of the column of phlogiſton in my new table of attractions; what it cannot do ſuddenly, is however done gradually by the intervention of favourable circumſtances.

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* Mr. Scheele firſt diſcovered the acid of Fluor; and he gave a very intereſting Memoir on Manganeſe (*Molybdænum Magneſium*, *Linn.*) in 1774, among the Memoirs of the Academy of Sciences at Stockholm.

How intereſting the more refined Chemiſtry is, will not be neceſſary to prove more circumſtantially on the preſent occaſion. It requires a great deal of prejudice or the greateſt ignorance, to treat it with contempt under the ſpecious pretence of unneceſſary refinement and ſubtleties. Earth, Water, Air, Heat, Light, and many more ſuch ſubtle ſubſtances are every where to be met with; and as long as their properties remain unknown, the effects both of nature and art, muſt remain involved in the moſt perfect obſcurity. In Chemiſtry, there are none of the *veritates otioſæ*; the leaſt phænomenon, when examined in all its cauſes, is always connected with others of the greateſt importance; in ſuch a manner, that every thing is ſhewn connected in the great œconomy of nature.

Finally, it is neceſſary to obſerve, that the following intereſting performance, has been ready for the preſs very near two years ago; though it is publiſhed only at preſent, for ſeveral reaſons, which it would be ſuperfluous to alledge here. By this delay it happened, that Dr. Prieſtley not knowing any thing of Mr. Scheele's work, has previous to its publication, deſcribed ſeveral new properties of Air. But they are here exhibited in a different manner, as well as in a quite different connection.

To conclude, I wiſh the Author an uninterrupted and laſting enjoyment of health; as he without doubt, will not be deficient in zeal, reflection, diligence, and opportunities to continue his reſearches; in which caſe the ſcience of nature, muſt expect from him many more inveſtigations.

TORBERN BERGMAN.

Upſal, July 13. 1777.

Naturalem cauſam quærimus & aſſiduam, non raram & fortuitam. SENECA.

OBSERVATIONS AND EXPERIMENTS ON AIR AND FIRE.

I

THE object and chief buſineſs of Chemiſtry conſiſts in a ſkilful decompoſition of bodies into their integrant parts, and in combining them again in various manners.

Only thoſe can be ignorant how difficult it is to execute this buſineſs with the greateſt accuracy, who either never undertook it, or never beſtowed upon it the required degree of attention.

2.

Hitherto chemiſts are not agreed upon the number of ſimple principles or elements, of which all corporeal ſubſtances are compoſed. This indeed is one of the moſt difficult problems; and ſome are of opinion, that there are no hopes of ever inveſtigating the elements of bodies; which circumſtance is but a poor comfort for thoſe, who delight in the inveſtigation of nature: but far be it, that the noble ſcience of Chemiſtry ſhould be circumſcribed in ſo narrow limits. Others believe that *earth* and *phlogiſton* are thoſe principles, which are the conſtituent parts of all corporeal ſubſtances. The greateſt number ſeem to admit only the peripatetic elements.

3.

I confeſs to have beſtowed a great deal of trouble upon the ſubject, in order to acquire diſtinct ideas. It is aſtoniſhing how numerous and varied the notions and conjectures of authors are in giving a definition of the fiery phænomena, which were the chief object of my inveſtigations. I found the great neceſſity to know the Fire, ſince it is impoſſible to make experiments without heat and fire, or to produce any effects with any *ſolvent*; I began to ſet aſide all the definitions of Fire; I made numerous experiments, in order to inveſtigate, if poſſible, this glorious ſubject. I ſoon found, that without knowing the *Air*, it is impoſſible to form a true judgment on the phænomena of Fire. After a ſeries of experiments I obſerved, that *Air really makes part of the compound of Fire, and is a conſtituent part of the flame and of ſparks.* I concluded, therefore, that a Treatiſe on Fire, could not be drawn up with any degree of accuracy, without an enquiry into Air.

4.

Air is that inviſible fluid, which is conſtantly breathed by us, which every where ſurrounds the globe, and which is poſſeſſed of elaſticity and weight. It is conſtantly filled with a great quantity of various effluvia, which are divided ſo minutely throughout its whole maſs, that they ſcarcely become viſible in the very rays of the ſun. Among theſe heterogeneous particles, watery vapours are always moſt prevalent. But it is mixed with another elaſtic fluid ſimilar to air, and which is in many reſpects differing from it, called very properly by Profeſſor Bergman, *Aërious Acid*. It owes its exiſtence to the deſtruction of organic bodies by putrefaction or combuſtion.

5.

Nothing has of late ſo much engaged the attention of natural philoſophers, as this ſubtle acid, or fixed air. It is no wonder, ſince inferences drawn from the properties of this elaſtic aicd, are by no means favourable to all thoſe who are poſſeſſed by prejudice. The partiſans of the Paracelſian doctrine are of opinion, that air is properly not ſubject to alteration or change: others think with Hales, that it may be mixed with other bodies; but that it then loſes its elaſticity; which it again recovers, as ſoon as it is diſengaged either by fire or fermentation. But obſerving, that this diſengaged air is poſſeſſed of properties widely different of thoſe in common air; they conclude, (without being founded on experience) that this Air is charged with heterogeneous particles; and that the Air ought to be cleared of the heterogeneous matter by ſhaking and filtrating it with various fluids. I believe this opinion might be adopted without the leaſt heſitation, if it only could be clearly proved by experiment, that a certain quantity of common Air may be changed, by the mixture of adventitious ſubſtances, into fixed or other Air: but as this has not as yet been put into execution, I ſuppoſe myſelf right, by adopting as many kinds of Air, as are extricated by experience. For having collected an elaſtic fluid, and obſerving that its elaſticity is increaſed by heat and diminiſhed by cold; when at the ſame time it retains conſtantly its elaſtic fluidity, and has properties and relations quite different of thoſe, which are found in common Air, I think myſelf entitled to conclude, that this is a peculiar kind of Air. I aſſert that ſuch collected Air ought to preſerve its elaſticity even in the moſt intenſe cold; ſince otherwiſe innumerable kinds of Air muſt be allowed; as it is highly probable that all ſubſtances may be changed into a vapour ſimilar to Air by an intenſe heat.

6.

Subſtances undergoing either putrefaction or decompoſition by Fire, diminiſh or as it were abſorb part of the Air. Sometimes it happens that they conſiderably increaſe the bulk of the Air, and in other inſtances, theſe ſubſtances will neither increaſe nor diminiſh a certain given quantity of Air; all which are very remarkable phænomena. Surmiſes cannot determine any thing with certainty, and they will ſtill leſs ſatisfy a chemical natural philoſopher, who always wants demonſtrative proofs. This clearly ſhews the neceſſity of making experiments, in order to elucidate this myſtery of nature.

7.

General Properties of common Air.

1. FIRE burns for a certain time in a given quantity of Air. 2. If the Fire does not yield during combuſtion a

fluid ſimilar to air, after the ſpontaneous extinction of the Fire, air is diminiſhed between a third and a fourth of its bulk. 3. It is not miſcible with common water. 4. All kind of animals live but a certain time in a given quantity of confined air. 5. Seeds, as for inſtance peas, will ſtrike roots, and grow to a certain height in a given quantity of equally confined air, by the addition of ſome water and moderate heat.

Hence if a fluid be exhibited ſimilar in all external appearances to Air, but which upon examination wants the enumerated qualities, (ſhould even only one be wanting,) I ſhould think myſelf convinced, that it is not common air.

8.

Air is a Compound of two Kinds of elaſtic Fluids.

Experiment I.

I diſſolved one ounce alcaline liver of ſulphur in eight ounces of water; of this ſolution I poured four ounces into an empty bottle, whoſe capacity was twenty-four ounces, and corked it well; then I turned the bottle, immerſed its neck into a ſmall veſſel with water, and kept it in this poſition a fortnight. The ſolution had partly loſt its red colour, and ſome ſulphur had been precipitated from it, during this time: after this I put the bottle in the ſame poſition in a larger veſſel with water, keeping the mouth and neck under water, and the bottom of the bottle above the water, and thus I drew the cork under water, which immediately ruſhed with violence into the bottle; I corked the bottle, pulled it out from under the water, and weighed the liquor contained, which I found to be ten ounces: if you ſubſtract out of it the four ounces ſolution of liver of ſulphur, there remain ſix ounces; conſequently, the experiment proves that, in a fortnight, out of twenty parts of Air, ſix parts were loſt.

9.

Experiment II.

a. I repeated the above experiment with the ſame quantity of liver of ſulphur, only with this difference, that I kept the bottle well corked during one week. I found hereupon, that out of twenty parts of air, no more than four were loſt.

b. Another time I kept the bottle ſtanding four months; the ſolution was of a dark yellow colour; but there had been loſt no more Air than in Experiment I. viz. ſix parts.

10.

Experiment III.

I mixed two ounces of cauſtic lixivium, made of alkali of tartar and quicklime, (ſuch as did not precipitate limewater), with half an ounce of the above ſolution of liver of ſulphur, which likewiſe did not precipitate any thing out of limewater. This mixture was of a yellow colour. I poured it in the ſame bottle as before (Experiment I.); and after it had been kept for a fortnight, well corked, I found it colourleſs, and without any precipitate. That the air had been likewiſe diminiſhed in the bottle, I collected from the ruſhing of the air into the bottle, by a ſmall hole in the cork.

Expe-

11.

Experiment IV.

a. I took four ounces of a ſolution of ſulphur in limewater, poured it into a bottle, and corked it well. After a fortnight the yellow colour was gone, and out of twenty parts of air, four were loſt. The ſolution contained not any ſulphur, but a powder had been precipitated, which was chiefly plaiſter of Paris.—*b.* Volatile liver of ſulphur diminiſhes likewiſe the bulk of the air.—*c.* But ſulphur and volatile ſpirit of ſulphur are not ſubject to any change in it.

12.

Experiment V.

I ſuſpended linen rags, dipped in a ſolution of alkali of tartar, over burning ſulphur. The alkali having been ſaturated by the volatile acid, I put the rags into a retort, and tied the opening carefully with a wet bladder. After three weeks I found the bladder very flaccid. I turned the retort and kept its mouth under water, having made a hole into the bladder; the water ruſhed with violence into the retort, and filled a fourth part of it.

13.

Experiment VI.

I collected in a bladder nitrous air which had been formed by a ſolution of metals in nitrous acid, and having well tied the bladder, I put it into a retort, and tied its mouth carefully with a wet bladder. The nitrous air gradually loſt its elaſticity, the bladder became flaccid and yellow, as if corroded by aquafortis. A fortnight after, I turned the mouth of the retort, kept it under water, and made a hole in the bladder; the water ruſhed into it, and no more than two-thirds of it remained empty.

14.

Experiment VII.

a. I immerſed the mouth of a retort in a veſſel with oil of turpentine, which was riſing every day into the retort a few lines. After a forta fortnight the fourth part of the retort was filled with it; I let it ſtand for three weeks more, but the oil would riſe no more. All ſuch oils as will dry in the air and may be changed into a reſinous ſubſtance, have this quality; but oil of turpentine, and linſeed oil will riſe eaſier, having previouſly rinced the retort with a concentrated ſharp lie.—*b*. I poured two ounces of perfectly limpid and colourleſs animal oil of dippel into a bottle, and cloſed it very carefully: in two months time it was thick and black. I then turned the bottle under water and drew the cork, when a fourth part of the bottle was filled with water.

15.

Experiment VIII.

a. I diſſolved two ounces of martial vitriol in thirty-two ounces of water, and precipitated the ſolution with a cauſtic lie. The precipitate being quite ſettled, I decanted the clear liquor, and I put the deep-green precipitate of iron, together with the remaining liquor into the bottle mentioned before, (No. 8.) and corked it well. After a fortnight,

night (during all which time I ſhook frequently the bottle) the green calx of iron had acquired the colour of *crocus martis*, and out of forty parts of Air twelve were loſt.—*b.* By moiſtening iron filings with ſome water, and keeping it cloſely ſhut in a bottle, likewiſe ſome part of the Air is diminiſhed.—*c.* The ſolution of iron in vinegar has the ſame effect upon Air. The vinegar precipitates the diſſolved iron in the form of a yellow *crocus*, and is entirely freed from the metal.—*d.* The ſolution of copper made by the marine acid in a cloſe veſſel, diminiſhes likewiſe the Air. In all the abovementioned kinds of Air neither a candle will burn, nor the leaſt ſpark be viſible.

15.

It appears from all theſe Experiments, that in each of them phlogiſton, the ſimple inflammable principle, is preſent. It is well known, that Air attracts the inflammable part of bodies, and deprives them of it: not only this may be ſeen from the above Experiments; but it alſo appears that in the tranſition of what is inflammable principle into the Air, a conſiderable part of the Air is loſt; but that what is inflammable principle is the ſole cauſe of this effect, is evident from No. 10, where not a veſtige of ſulphur remained, ſince the colourleſs *lixivium*, according to my Experiments, contained only ſome vitriolated tartar; and No. 11. proves this likewiſe.

But ſince ſulphur by itſelf, and likewiſe the volatile ſulphureous acid have no effect upon air, according to No. 2. it is evident, that the decompoſition of liver of ſulphur is done by the laws of a double affinity, *viz.* the alkalis and lime attract the vitriolic acid, and Air attracts their phlogiſton.

From the above Experiments it likewiſe appears, that a given quantity of Air can be united to, or ſaturated as it were only by a certain quantity of phlogiſton, which is evident from No. 9. letter *c.* But whether the phlogiſton which is loſt from the ſubſtance, ſtill remained in the reſiduum of Air in the bottle; or whether the loſt Air was united, and became fixed with the ſubſtances of liver of ſulphur, oils, &c. are queſtions of great importance.

It would follow from the *firſt* queſtion, that phlogiſton has the quality of depriving

Air of part of its elaſticity, and that for this reaſon it is more compreſſed by the external air. In order to extricate myſelf from theſe doubts, I firſt ſuppoſed that ſuch air ought to be ſpecifically heavier than common Air, both on account of the phlogiſton it contained, and of its greater denſity. But how much was I aſtoniſhed, upon finding, that a very thin retort, filled with this kind of Air, and weighed in the niceſt manner, was not only not heavier than an equal meaſure of common Air, but even ſomewhat lighter.—I then imagined, that the *laſt* queſtion might be applicable; and then it would follow, that the loſt Air might again be ſeparated from the materials employed in the Experiment. None of the Experiments ſeemed to intimate this more clearly, than that in No. 10; becauſe this reſiduum, as mentioned before, conſiſted of vitriolated tartar and alkali. In order therefore to try whether the loſt Air might not be changed into fixed Air; I examined whether this would not appear, when ſome part of this cauſtic lie was poured into limewater, but all was in vain, and no precipitate could be procured. I tried by ſeveral other methods to extricate the loſt Air from this alkaline mixture, but ſince the ſucceſs was the ſame with the preceding one, I forbear mentioning the Experiment, to avoid prolixity.—Thus much I ſee from the abovementioned Experiments; that Air is compoſed of two different fluids, the one of which attracts not the phlogiſton, and the other has the quality of attracting it, and this latter fluid makes between a third and a fourth of the whole bulk of the air. What becomes of this laſt kind of air, after it unites with the phlogiſton, is a queſtion which ought not to be decided by ſurmiſes, but by new experiments.

Let us now examine how air is affected by inflammable ſubſtances, when expoſed to fiery motion. Firſt let us conſider that kind of fire, which, during combuſtion, yields no fluid ſimilar to air.

17.

Experiment I.

grains

I put into a thin retort, whoſe capacity was of thirty ounces water, nine ounces of urinous phoſphorus, and ſhut its mouth up carefully. Then I heated, by a burning candle, the place of the retort, where the phoſporous lay, which latter ſoon began to melt, and immediately after to burn; the retort filling with white fumes, which ſtuck to the ſides of the veſſel like white flowers; This is the acid of phoſporus in its dry ſtate. After the retort had grown cold, I held it inverted, under water, and drew the ecork there; ſcarcely was this done, when the external air preſſed the water into the retort; this water weighed nine ounces.

18.

Experiment II.

Having put into the ſame retort ſome pieces of phoſphorus, I put it by, well ſhut up during ſix weeks, or ſo long till it would no more caſt a light; when I obſerved that one-third of the bulk of air had been loſt.

19.

Experiment III.

I put three tea-ſpoonfuls of ſteel filings into a phial, of the capacity of two ounces of water; I poured on it one ounce of water, and gradually added half an ounce of oil of vitriol. A violent efferveſcence and fermentation enſued; after the ſcum had ſubſided, I ſhut the orifice as cloſely as poſſible with a cork, through which a glaſs tube had been previouſly put. A (Fig. 1.) This phial I placed in a veſſel with hot water, B B, (for cold water would much hinder the ſolution) then I approached a burning candle to the orifice of the glaſs tube, when immediately the inflammable air took fire, and burnt with a yellowiſh green flame. Immediately after this, I took a ſmall retort, of the capacity of twenty ounces water, C, and kept it ſo deep under water, that by this means the ſmall flame came juſt into the middle of the retort: a moment after the water began to mount in the retort, till it reached to D, when the flame was extinguiſhed; the inſtant after, the water began to lower, and was entirely expelled out of the retort. The ſpace till D contained four ounces, conſequently the air was diminiſhed about one-fifth.—I poured a few ounces of limewater into the retort, in order to ſee whether, during the burning, ſome aerial acid (*i. e.*) fixed air) had not been extricated: but I found none. I repeated the experiment with filings of zinc, and the reſult was in every reſpect the ſame. The conſtituent parts of this inflammable air I ſhall hereafter demonſtrate; for though it might appear from theſe experiments, that it was chiefly phlogiſton, there are, however, other experiments againſt it.

Now let us examine the relation of air to that kind of fire, which, during combuſtion, yields a fluid ſimilar to air.

20.

Experiment IV.

It is known that the flame of a candle abſorbs air; but ſince it is very difficult, or next to impoſſible, to light a candle in a cloſe retort, previouſly an experiment was made as follows: I put a burning candle into a diſh filled with water, and then covered the candle with an inverted retort: immediately after this, great air bubbles roſe out of the water, which were cauſed by the expanded air from the heat in the retort; when the flame decreaſed in ſize the water began to mount into the retort; after the flame was extinguiſhed, and the retort grown cold, I found its fourth part filled with water.—This experiment was not deciſive to me, ſince I was not ſure, whether this fourth part of air was not expelled by the heat of the flame; in which caſe the external air preſſing neceſſarily on the water, after the cooling of the retort, endeavours to reſtore the equilibrium, and forces a quantity of water into the retort equal in bulk to the air which had been expelled by heat. I therefore made the following experiment.

21.

Experiment V.

a. I faſtened to the bottom of a deep diſh A (fig. 2.) a tough maſs, compoſed of melting wax, roſin, and turpentine, of the thickneſs of about two fingers: in its middle I fixed a ſtrong iron wire, reaching up to the middle of the retort B. On the point of the wire C a ſmall wax candle was fixed, whoſe wick I had myſelf made by twiſting three ſmall threads together; this candle was lighted, and the inverted retort B was immediately put on it, and preſſed deep into the maſs below; and then the diſh was filled with water. After the flame was extinguiſhed, and every thing perfectly grown cold, I opened the mouth of the retort under water, when two ounces of water ruſhed in. The retort was cable of holding one hundred and ſixty ounces of water. The air diminiſhed therefore muſt have been two ounces. Had this air been abſorbed by the inflammable matter; or had the heat of this little flame expelled juſt ſo much air, before I could ſtop the mouth of this retort by preſſing it into the tough ſubſtance below? The latter ſeems to be the caſe, as I collect from the following circumſtance: I took a ſmall matraſs, or retort, of about twenty ounces meaſure; in this I left a candle burning in the manner mentioned before: After every thing was grown cold, about two ounces of water ruſhed in. Had the firſt two ounces meaſure of air been abſorbed, there muſt have been in this experiment abſorbed only two drachms meaſure of air.

b. I repeated the above experiment with the large matraſs, only with this difference, that I uſed ſpirit of wine inſtead of a candle. For I fixed three iron wires, of equal length, into the abovementioned tough maſs at the bottom of the diſh, reaching to the middle of the matraſs; on theſe wires a ſquare piece of tin was laid, and then a ſmall ſaucer with ſpirit of wine was put on the tin; the ſpirit was ſet fire to, and the matraſs was ſet over the whole apparatus. After the whole was grown cold, I found that three ounces meaſure of air had been expelled by the heat of the flame.

c. On the ſame apparatus I put ſome ſmall live coal, and left them to extinguiſh under the retort. After growing cold, I found that the heat of the coals had expelled three and a half ounces meaſure of air.

Theſe experiments ſeem to prove, that the tranſition of phlogiſton into the air diminiſhes not always its bulk; which however other experiments clearly indicate, which are mentioned No. 8—16: but what we now are going to mention, demonſtrates that the part of air, which unites with the in-

inflammable, and is in a manner abſorbed, is again reſtored by the re-extricated aerial acid (fixed air.)

22.

Experiment VI.

I poured in each matraſs, after the fire was extinguiſhed, in the abovementioned experiments (No. 21. *a. b. c.*) and quite grown cold, ſix ounces of *(lac calcis)* milk of lime, (*i.e.* lime-water, which contained more quicklime than the water could diſſolve), I then carefully ſhut with the hand the mouth of the matraſs, and ſhook repeatedly the liquor in it; after which the inverted matraſs was immerſed in water, and the hand a little taken away, ſo as to make a ſmall opening, when immediately the water ruſhed into the matraſs; hereupon I ſhut again the mouth of the matraſs with my hand, and ſhook it ſeveral times, immerſed it in water, and made an opening; this manœuvre I repeated twice over, until the water would no more ruſh into the matraſs, or until no more aerial acid (fixed air) was in the matraſs. I obſerved that in each experiment, about ſeven or eight ounces of water mounted into the retort, and conſequently a nineteenth part of air was loſt. This is no doubt ſomething; but ſince in the combuſtion of phoſphorus (No. 17.) very nigh the third part of air was loſt; there muſt be ſtill another cauſe exiſting, why the ſame quantity in proportion here was not abſorbed. It is known, that one part of aerial acid (fixed air) mixed with ten parts of common air, extinguiſhes the fire; and here, in this inſtance, are moreover the watery vapours, which ariſe from the deſtruction of the oily ſubſtance, and are expanded by the heat of the flame: theſe two elaſtic fluids ſeparating from the flame, no doubt hinder the fire very much from burning longer, (though it would do it otherwiſe) ſince there is no current of air exiſting, which might carry them off from the flame. After the aerial acid has been ſeparated from this air, by the *lac calcis*, a candle can again burn in it; though it is only for a very ſhort ſpace of time.

23.

Experiment VII.

I put a crucible on the ſtand (No. 21. *b*) filled with ſulphur, ſet it on fire, and covered the matraſs over it. After the ſulphur could burn no longer, and every thing had grown cold; I found that two parts out of one hundred and ſixty, were expelled from the matraſs by the heat of the flame. Then I poured ſix ounces of clear limewater into the matraſs, and began to ſhake it, as mentioned above; and obſerved that the ſixth part of all the air was loſt by burning. The limewater cauſed no precipitate; which ſhews that ſulphur, during its conflagration, yields no aerial acid, but a ſubſtance ſomewhat analogous to air, or the volatile ſulphureous acid, which occupies the vacuum cauſed by the union of the phlogiſton with air. Certainly it is a remarkable circumſtance to obſerve, that the phlogiſton ſeparated from bodies, either without or with a fiery motion, and united with air, always conſiderably diminiſhes the bulk of air.

24.

Experiments, proving that common air, conſiſting of two kinds of elaſtic fluids, after having been ſeparated by means of phlogiſton, may again be compounded.

I have already mentioned (No. 16.) that I could not find again the loſt air. It might be objected, that the loſt air is ſtill contained in the reſiduum of air, which could not farther be united with the phlogiſton; for finding that kind of air lighter than common air, it might be ſuppoſed that the phlogiſton when united with this air made it leſs ponderous, which circumſtance is already known from other experiments. However, ſince phlogiſton is a ſubſtance, (which always ſuppoſes ſome weight,) I very much doubt whether this hypotheſis be founded on truth. But without entering into any detail, I will prove, that by the union of air with the inflammable principle, a compound is formed, ſo ſubtle as to paſs through the fine pores of the glaſs, and diſperſe all over the air.

25.

Chemiſts have frequently diſtilled the fuming ſpirit of nitre, from oil of vitriol ſpread on ſalt-petre; and they could not help obſerving, that this acid is in the beginning of the operation red; in the ſequel white and colourleſs; and at laſt again ſo intenſely red, that it is impoſſible to look acroſs the receiver. Here it ought to be remarked, that in caſe the heat is much increaſed towards the end of the diſtillation, the whole mixture is put into ſuch an effervеſcence, that every thing paſſes over into the receiver; and during this effervеſcence a kind of air is diſengaged, which deſerves every attention. —If for this operation a very *black oil* of vitriol is employed, the acid going over in the beginning is not only by far more red, than if a *white oil* of vitriol had been uſed; but alſo by putting a burning candle into the receiver, after the paſſing over of about one ounce of acid, you ſee it inſtantly extinguiſhed; whereas a burning candle introduced towards the end of the operation when the mixture is in effervеſcence, into the receiver filled with intenſely red vapours, not only continues burning, but its flame becomes infinitely more vivid, than in common air. The ſame phenomenon appears, if towards the end of the operation a receiver filled with air, wherein a candle will not burn, is luted to the apparatus; for after it has been fixed about half an hour, a candle will again burn in the air it then contains.

The previous queſtion therefore ought to be put: *whether the vapours of acid of nitre are naturally red?* I beg leave to introduce here this queſtion; ſince I have reaſon to believe that there are people, who make the red colour a characteriſtic of this acid. The colours of the acid of nitre are accidental: for if ſeveral ounces of fuming ſpirit of nitre be diſtilled by a very moderate heat, the yellow colour ſeparates from it, and paſſes over into the receiver, and the reſiduum in the retort is as white and colourleſs as water. This acid has all the chief qualities of acid of nitre, and the yellow colour alone is wanting: This I would call the *pure* acid of nitre. No ſooner comes it in contact with an *inflammable*

mable principle, than it grows more or leſs red. The red acid is more volatile than the pure, becauſe moderate heat alone will ſeparate it; and for this very reaſon in the diſtillation of Glauber's ſpirit of nitre, the volatile ſpirit goes over firſt, and after it has paſſed the uncoloured follows. However, why does the acid again paſs over intenſely red at the end of the diſtillation? why was the red colour not gone off in the beginning of the operation? whence obtains it phlogiſton at that period? Here lies the difficulty.

I mentioned in the preceding number, that a burning candle is extinguiſhed in the receiver at the beginning of the diſtillation. The cauſe of it is alledged in the experiment already deſcribed, No. 13. The acid of nitre paſſing over in the form of vapours, attracts the phlogiſton, which diſcovers itſelf in the black colour of the oil of vitriol, immediately after it meets theair; which again, by elective attraction, deprives the phlogiſticated acid of its inflammable principle; by which means part of the air contained in the receiver is loſt, whence flame immerſed into it is extinguiſhed. (No. 15.)

§ 26 missing

27.

Acid of nitre can take up phlogiſton in various proportions, and obtains at each proportion different qualities. *a.* If it be, as it were, ſaturated with it, real fire riſes, and it is entirely deſtroyed. *b.* If the inflammable principle be leſs copious, the acid is converted into a kind of air, which will neither unite with alkalies, nor with abſorbent earths, and with water only in ſmall proportions: if this acid of nitre analogous to air, meets the air, the latter attracts the phlogiſton, and loſes its elaſticity. (No. 13.) The vapours become red, the air undergoes likewiſe this remarkable and natural change, of being not only diminiſhed, but alſo of growing warm. *c.* If the acid of nitre receives a ſtill leſs proportion of phlogiſton, it is likewiſe changed into a kind of air; which, like common air is inviſible *, but capable of uniting with alkalies and terreous ſubſtances, and of yielding by their mixture true neutral ſalts; however, this phlogiſticated acid ſo cloſely adheres to theſe abſorbent ſubſtances, that even the addition of vegetable acids can not expel it. In this manner it is to be met with it, in ſaltpetre, made red hot; and likewiſe in the *nitrum antimoniatum*. If this acid of nitre meets the common air, the latter loſes its elaſticity, and is diſſolved in red vapours; if mixed in certain proportions with water it becomes tinged, blue green or yellow. *d.* If the pure acid of nitre receives but a very ſmall proportion of inflammable principle, the vapours become only tinged red, they are deprived of elaſticity; but this acid of nitre is however become more volatile than a pure acid: And this ſmall quantity of plogiſton ſo firmly adheres to the acid, that even air, which has the ſtrongeſt affinity to phlogiſton, is incapable of extricating the ſame.

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* The author means to ſay, that as common air has not particles, which cloſe to them, or in ſmall proportions become diſcernible to the naked eye; in the ſame manner, the air formed by the addition of a ſmall proportion of phlogiſton to acid of nitre, becomes not diſcernible to the naked eye; though its effects and qualities may ſufficiently characteriſe it, ſo as to diſtinguiſh it from other kinds of air. (F.)

28.

Suppoſing then all this; let us try, whether the peculiar phenomenon, mentioned, No. 25, (*viz.* that fire burns ſo very clear in the receiver at the end of the diſtillation) may not be accounted for, Is not that peculiar kind of air, eſpecially that peculiar part of it by the medium of which fire burns, which (according to the preceding experiments) conſtitutes only the third part of common air; is not this kind of air introduced into the receiver by diſtillation? Muſt not this kind of air, after its union with phlogiſton and its thereby becoming inviſible to the eye, re-appear; whenever it comes in contact with a ſubſtance, which has a *ſtronger* affinity to inflammables, than this kind of air? Is there any rational perſon that will doubt, that this exactly happens in the diſtillation of fuming ſpirit of nitre? Did I not obſerve in the preceding No. 27. *d.* and does not daily experience corroborate it, that the more or leſs intenſe redneſs cannot be eaſily ſeparated from the acid of nitre to which it is united, though air attracts ever ſo much the ſuperabundant phlogiſton of this acid; and in this attraction a ſenſible degree of heat is produced (No. 27. *b*) whence I am induced to ſuſpect, that during each union of the phlogiſton with air, heat is generated; and conſequently that *heat* is a compound of that air which makes the third part of common air, (No. 16.) and of an inflammable ſubſtance. This heat it is, which during the diſtillation of concentrated acid of nitre is decompoſed, and reſolved into its integrant parts. This owes its exiſtence to the fire employed in the diſtillation; at firſt it is compoſed of air, *(without which no fire can be imagined,)* and of the phlogiſton of the coals; it penetrates the cuppel, the ſand and retort, where it meets with a ſubſtance attracting more powerfully the phlogiſton, than the air does which is united with it, conſequently heat is decompounded; by which means the acid of nitre is tinged of a deep red colour; the air which had been divided into incomprehenſibly minute parts, reaſſumes its former quality; it is puſhed into the receiver by the concomitant acid, becomes more elaſtic by heat, where it has opportunity again to attract phlogiſton; and ſince in the receiver a greater proportion of this kind of air is preſent, than in common air, it is no wonder, that the flame there is ſtronger and brighter.—This opinion ought to appear to my readers as ſtrange, as it did in the beginning to me. However, being ſince convinced, that it is not a mere hypotheſis, but one of the cleareſt truths, I ſhall endeavour to corroborate it by further emperiments.

29.

I took a glaſs retort, capable of containing eight ounces of water, and diſtilled fuming ſpirit of nitre according to the uſual method. In the beginning the acid paſſed over red, then it became colourleſs, and laſtly again all red: no ſooner did this happen, than I took away the receiver; and tied to the mouth of the retort a bladder emptied of air, which I had moiſtened in its inſide with milk of lime *lac calcis*, (*i. e.* lime-water, containing more quicklime than water can diſſolve) (No. 22.) to prevent its being corroded by the acid. Then I continued the diſtillation, and the bladder gradually expanded. Hereupon I left every thing to cool, tied up the bladder, and took it off from the mouth of the retort.—I filled a ten-ounce glaſs with this air (No. 30. *c.*) and put a ſmall burning candle into it; when immediately the candle burnt with a large flame, of ſo vivid a light a light that it dazzled the eyes. I mixed one part of this air with three parts of air, wherein fire would not burn; and this mixture afforded air, in every reſpect ſimilar to the common ſort. Since this air is abſolutely neceſſary for the generation of fire, and makes about one-third of our common air, I ſhall henceforth, for ſhortneſs ſake call it *empyreal air*, [literally *fire-air*:] the air which is unſerviceable for the fiery phenomenon, and which makes about two-thirds of common air, I ſhall for the future call *foul air* [literally *corrupted air*].

30.

It may happen, that I ſhall be aſked, in what manner I pour air from one veſſel into another? I find it therefore neceſſary to deſcribe this previous to anyother thing.—My apparatus and veſſels are the moſt ſimple that can poſſibly be found. Matraſſes, retorts, bottles, phials, and ox-bladders are the things which I employ. The bladders are, when freſh rubbed and well blown up, bound up tight, and hung for drying. If I want a bladder, and find it ſtill as well expanded as originally, I may be aſſured that it is tight.

a. If I want to collect a certain kind of air, for inſtance, phlogiſticated acid of nitre; (No. 13.) I take a ſoft bladder, rubbed on its inſide with a few drops of oil, I put into it the filings of ſome metal, iron, zinc or tin; I then carefully expel, by compreſſion, all the air contained in it, and tye the bladder to the mouth of a ſmall phial, into which I had poured ſome aquafortis; I unfold the bladder a little, that ſome of the ſteel filings may fall into the aquafortis; and in proportion as they are diſſolved the bladder is expanded. When I have a ſufficiency of ſuch air, I tye the orifice of the bladder pretty cloſe with a ſtring, and take it off from the phial.

b. If the phlogiſticated acid of nitre be mixed with aërial acid, which happens if the acid of nitre is diſtilled from ſugar, I then tye a bladder ſoftened in water round the extremity of the neck of the retort (A, fig. 3.); but the better to preclude the eſcaping of air, it is neceſſary to ſcratch in that place the neck of the retort with a flint. (Retorts which I employ for this operation, I always order to be made of the ſize of half an ounce, and others increaſing to the ſize of three ounces, with a neck of the length of about a foot; that the bladder, tied to its neck, may not ſuffer from the heat of the oven, nor from the hot vapours during the operation). In this bladder I pour ſome *(lac calcis)* milk of lime (No. 22.) and expel air as carefully as is poſſible. This lime abſorbs, during diſtillation, the aerial acid (fixed air) and leaves the phlogiſticated acid of nitre.

c. In the ſame manner as deſcribed (*a.*) I collect the aerial acid, and the inflammable air from ſulphur (of which I ſhall ſpeak in the ſequel). If the bladders be moiſt, or even only the ambient air ſo, theſe two kinds of air penetrate through the bladder; but if on the contrary, the air be dry as well as the bladder, this does not happen. Inflammable air from metals, (viz. iron, zinc, &c. &c.) I obtain in the ſame manner, except that the glaſs is put into warm ſand. This air is ſtill more ſubtle than the preceding, and in a few days it paſſes through the ſmall pores of the bladder, though the air and bladder be ever ſo dry. I often experienced this to my diſappointment. — *d.* I often collect air in bladders, without any phial. I put

I put into a ſoft bladder (A A. fig. 4.) that ſubſtance, from which I intend to extricate the air, as for inſtance, chalk; above this chalk I gather the bladder, and faſten it by means of a ſtring B B; I then pour on it the acid diluted with water, and expreſs the air as carefully as poſſible out of the bladder; laſtly, I tye up the mouth of the bladder. Then I untye the ſtring B, and admit the acid upon the chalk, which extricates the aereal acid, and expands the bladder.—*e*. If I deſire to introduce a certain kind of air out of the bladder into a retort or bottle, the veſſel ought to be filled with water, and cloſely ſhut up with a tight cork; I then tye the orifice of the bladder (C to D. fig. 4.) including the air cloſely to the mouth of the bottle; this done, I turn the bottle, ſo that the bladder is below and the bottle uppermoſt; I hold the bottle with the left hand, and draw the cork with the right, keeping the cork in the bladder between two fingers, until the water is gone out of the bottle into the bladder, and the air out of the bladder into the blottle; after which I ſhut up the mouth of the bottle with the cork, and untye the bladder from the bottle.

If I intend to keep this air for ſome time, I immerſe the inverted bottle to its neck in a veſſel with water.

f. If the bladder contains aerial acid, or another kind of air, which may be abſorbed by water, and I intend to unite it with water in a dextrous manner; I fill a bottle with cold water, and having tyed the bladder to the mouth of the bottle, I then pour out of the bottle about the fourth part of the water into the bladder, and ſhut again the mouth of the bottle with the cork I had held all that time between my fingers in the bladder. I then gently ſhake the bottle, by which means the water imbibes the air: Hereupon I open the cork ſomething and the air ruſhes into the bottle, in order to fill the ſpace become empty in the bottle, without ſpilling any water in the bladder; I ſhut again with the cork the bottle, and ſhake the water; and repeat the proceſs two or three times more, by which operation the water becomes ſaturated with that kind of air.

g. If I intend to mix two kinds of air in a matraſs or retort, I let run out of the retort into the bladder tyed to it, that proportion of water which I wiſh to have of that kind of air. I then tye the bladder, including the ſecond kind of air to the bottle, and let the remaining water run into this bladder; this being done, I ſhut the bottle with the cork.

h. If I intend to transfer air collected in a bottle, into a bladder, I reverſe the operation: *i. e.* I fill the bladder with that proportion of water which I wiſh to introduce of air into the bladder; this I tye up in one part of the bladder; then I fix the bladder to the mouth of the bottle, I untye the ſtring above the water, draw out the cork and let the water run into the bottle; tye again the bladder, ſeparate it from the bottle, and I have transferred the air from the bottle into the bladder.

i. If I have in a bottle two kinds of air mixed together, the one of which may be abſorbed either by water or lime, and I intend to aſcertain what proportion of each is contained in the mixture; I tye a bladder, filled with as much milk of lime as will fill the bottle, to the veſſel; draw the cork, and let the water or milk of lime run into the bottle; I then turn the bottle, and pour the milk of lime into the bladder, and repeat the operation ſeveral times: The quantity of the milk of lime remaining in the bottle aſcertains the quantity of the air which has been abſorbed.

Theſe are the methods employed by me in my enquiries on air. I confeſs that they will not pleaſe every body, ſince they decide not to a nicety: but in my enquiries they have all proved ſatisfactory: and often people pretend to ſplit even a hair, where there is no neceſſity to attend to minutiæ.

31.

Continuation of the Experiment mentioned (No. 28.*)* ; *with a demonſtration that heat or warmth is compoſed of phlogiſton and empyreal air.* (29)

It may be objected; that the air obtained according to (No. 28.) is perhaps only a dry acid of nitre changed into elaſtic vapours. However, if this opinion were founded in truth, this air would be not only corroſive, but alſo yield again ſaltpetre with alkali: And nevertheleſs this never happens. The above objection would have ſome weight, if I could not demonſtrate, that there are more ſubſtances yielding during diſtillation the ſame kind of air, which is generated from diſtilled acid of nitre. This demonſtration is not wanting.

I have proved in a paper on Manganeſe (*molybdænum magneſia* Linn.) (*Manganeſe* Cronſted's Mineralogy, ſect cxiii.) inſerted in the memoirs of the Swediſh academy of the year 1774, that this mineral ſubſtance is not ſoluble in any one of the acids; unleſs there be added an inflammable ſubſtance, which might communicate its phlogiſton to the manganeſe, and thus procure admittance to the acids. I there likewiſe obſerved, that acid of vitriol unites with pounded manganeſe, during a very violent diſtillation, and makes it ſoluble in water; and if this manganeſe be again ſeparated from the acid of vitriol by precipitation, the moſt evident proofs of the preſence of an inflammable ſubſtance are obſerved. From this phænomenon, I then collected, that there is a *phlogiſton in heat*; and this ſuſpicion has been confirmed. If I aſſert, that the acid of nitre decompounds heat, becauſe the acid has a greater affinity to inflammables, than empyreal air; and if I have collected from various qualities of the manganeſe, that it attracts the phlogiſton ſtronger than even acid of nitre; I cannot heſitate a moment believing that the manganeſe decompounds the heat, for the very reaſon for which acid of nitre does it. I could by no means doubt of this; ſince I had already ſeveral years ago obſerved, that if during the calcination of manganeſe with oil of vitriol in an open crucible, ſome duſt of coals is carried accidentally on the ſurface of this mixture; theſe ſubtle coals are inſtantaneouſly ſet a fire, with a very vivid ſplendor. —I in conſequence made the following experiments.

Experiment I.

32.

I mixed ſo much of concentrated oil of vitriol with manganeſe reduced to an impalpable powder, that it became a thick, ſtiff *magma* (or dregs.) This mixture I diſtilled in a ſmall retort by open fire. In lieu of a receiver I employed a bladder emptied of air; and, that the riſing vapours might not attack the bladder, I had poured into it ſome milk of lime. (No. 30 *b.*) As ſoon as the bottom of the retort became red hot, a kind of air paſſed over, which gradually expanded the bladder.

This air has all the qualities of a pure *empyreal air*.

Experiment II.

33.

By diſtilling two parts of an impalpable powder of manganeſe, with one part of acid of phoſphorus, in the ſame manner as mentioned in the preceding (No. 32.) I obtained likewiſe *empyreal air*.

Experiment III.

34.

a. I diſſolved the common *magneſia alba* in aquafortis; this ſolution I evaporated till it became dry. This ſalt I put into a ſmall retort for diſtillation, as mentioned (No. 32.). Before even the retort was red hot, acid of nitre was ſeparated from the magneſia alba in vapours as red as blood, and immediately the bladder became expanded: The air thus obtained was my *empyreal air*.

It is conſtantly obſerved that all metals diſſolved in acid of nitre, when again deprived by heat of this menſtruum, part with it in vapours as red as blood.

b. I diſtilled in the ſame manner mercurial nitre till the acid of nitre was ſeparated from the red precipitate: And I obtained likewiſe my *empyreal air.*—*Saltpetre* alone will decompound heat. Whence ariſes the boiling of the fuſed and red hot ſaltpetre in the crucible? neither ſmoke nor vapour are ſeen riſing, and coal duſt which is carried acroſs the open crucible is burning with a radiant light. Whence becomes the ſaltpetre moiſt and deliqueſcent, which has been kept for about half an hour in red hot fuſion, after it is grown cold, without giving any ſigns of an alkali? (No. 27. *c.*) What can be the cauſe, that this fuſed ſaltpetre, when ground with vegetable acids, will immediately part with its volatile acid? After becoming acquainted with the component parts of heat, it is not difficult to anſwer to theſe queries.—Had the Chemiſts of laſt century vouchſafed to examine more minutely the elaſtic fluids ſimilar to air, which appear in ſo many operations, we ſhould at preſent be farther advanced. They wanted to ſee every thing in corporeal form, and collect them as drops in the receiver. It has been better enquired into in our days; and air has been examined; and who is not ſenſible of the utility of the reſults of ſuch experiments?

I have declared that ſaltpetre alone will decompound the ſubſtance of heat.—The following experiment proves it.

Experiment IV.

35.

I put one ounce of ſaltpetre purified for diſtilling, into a glaſs retort, and employed a moiſtened bladder, emptied of air, in lieu of a receiver (fig. 3.) As ſoon as the ſaltpetre became red hot, it began boiling; and at that period of time the bladder was expanded from the air that paſſed over. I continued the diſtillation, till the boiling in the retort ceaſed; and the ſaltpetre was on the point of penetrating through the ſoftened glaſs retort. In the bladder I found the pure *empyreal air*, taking up the ſpace of fifty ounces meaſure.—This is the beſt and cheapeſt method of obtaining *empyreal air*.

36.

The preceding experiment might eaſily prevail upon a man to doubt, if the acid of nitre attracts the phlogiſton more powerfully, than *empyreal air*; what may be the cauſe, that the ſaltpetre leaves not off boiling, imbibing ſo much phlogiſton as is ſufficient to alkaliſe the ſaltpetre? The anſwer is already given, (No. 27. *d.*)

What paſſes through the retort beſides heat? If ſome one were inclined to believe that perhaps light might interfere, the contrary may be ſeen from the diſtillation of fuming ſpirit of nitre, and likewiſe from the experiment (No. 34. *a.*) For in theſe inſtances, neither the ſubſtances contained, nor the retort, were red hot, and nevertheleſs the *empyreal air* was obtained. Now let us examine, *whether there are not more ſubſtances exiſting, which are capable of decompounding heat*; and this inquiry will lead us to thoſe phænomena, which are obſerved in the calcination of noble metals by fire.

37.

That the acid of nitre as well as the dephlogiſticated marine acid, deprive the noble metals of the inflammable principle, which gives them their ſplendor; is a fact ſo well aſcertained, that I think it ſuperfluous to give any further proofs of it. The quantity of air bubbles appearing on the ſurfaces of gold, ſilver, and quickſilver in the acid of nitre, and which when burſting, as ſoon as they come in contact with air, ſhew a yellow tinge, (No. 27.) are evident proofs of this aſſertion.—Nevertheleſs, ſince it has been obſerved, that the calxes of metals ſeparated from their ſolvents, are reduced to metalleity without the addition of a phlogiſton, by mere heat; it has been inferred, that the corroding ſolvents were not capable of depriving the noble metals of their phlogiſton.—Others, who are convinced of the calcination of metals by acids, ſuppoſe that the ſmall deficiency of phlogiſton ſeparated from the coals, paſſes through the crucible, and is incorporated with the metallic calxes. This opinion approaches nearer to truth; however

ever it ought to be known that the phlogiſton does not ſeparate from any ſubſtance, unleſs the ſubſtance attracting the phlogiſton be brought into immediate contact with it. Hence it likewiſe appears, that ſince the phlogiſton of the coals has no action upon the crucible, and, of conſequence, comes not in contact with the metallic calx, and yet the reduction of it takes place; there ought to be ſome other ſubſtance which is to yield the phlogiſton wanting for the metallic calxes. Heat, conſidered as a ſimple, uncompounded ſubſtance, cannot bring about this reduction; ſince in that caſe the calxes of the ignoble or baſe metals, muſt be likewiſe reduced by it. But heat, conſidered as a compound, will not leave a doubt that it procures the reduction of the calxes of noble metals by means of its phlogiſton: and if this happens, *empyreal Air* muſt be extricated during the reduction, for the ſame reaſons which are alledged in the preceding *numeros.*—But to prove this evidently, I made the following Experiments:

38.

Experiment V.

I took a ſolution of ſilver made by acid of nitre, I precipitated it with ſalt of tartar, the precipitate I waſhed and dried; hereupon I put the calx of ſilver in a ſmall glaſs retort, for its reduction in open fire, and tied an empty bladder at the orifice of the neck of the retort. The bladder was immediately expanded from the Air paſſing over. After the end of the diſtillation, I found the calx of ſilver in the retort half melted, with its metallic ſplendor; however, the precipitation being made with ſalt of tartar, which always is united with a great quantity of aërial acid, and this acid uniting during the operation with the calx of ſilver; this acid was ſeparated by the milk of lime No. 30, letter *i.* the remaining half was of pure *empyreal Air.*

39.

Experiment VI.

I precipitated a ſolution of gold made in aqua regis, with ſalt of tartar; the edulcorated and dried calx of gold, I reduced after the preceding manner; I obtained here the ſame *empyreal Air*, except that no aërial acid came over; which is no wonder, ſince the ſaturated ſolution of gold cauſed an efferveſcence with the alkaline ſalt, which does not happen with the ſolution of ſilver.

40.

Experiment VII.

It is well known that the red precipitate of quickſilver receives again the form of *Mercurius currens* without the addition of an inflammable. However, ſince mercury moſt certainly is deprived of its phlogiſton by vitriolic and nitrous acid, it muſt neceſſarily reſume the phlogiſton, whenever it receives its metalliety.

a. Into a ſolution of corroſive ſublimate, a ſolution of ſalt of tartar was gradually poured by drops; I waſhed and dried the browniſh red precipitate thus obtained. Then I put it in open fire, in a ſmall retort, with an empty bladder at its neck, for reduction. When the calx became red-hot, the bladder was expanded, and the quickſilver mounted by the neck of the retort. The *empyreal Air* thus obtained, contained ſomewhat of aërial acid.

b. Mercury reduced into calx by acid of nitre, or the red precipitate, and treated in the ſame manner as in the former proceſs, gave the ſame reſult; only with this difference, the *empyreal Air* thus obtained was pure, without the addition of aërial acid.

41.

Experiment VIII.

I have demonſtrated in a memoir on arſenic, communicated by me to the Royal Academy of Sciences, at Stockholm, that this poiſonous ſubſtance conſiſts of an acid *ſui generis* and a phlogiſton. I remarked in that memoir, that this acid could be ſublimated into arſenic, by mere continued heat; though I then already underſtood the cauſe of it, I did, however, avoid mentioning it, for ſhortneſs ſake. I put ſome of the fixed arſenical acid in a ſmall retort for diſtillation, with a bladder tied to its neck.

When this acid was melted and red-hot, it began to boil; during which operation the arſenic was ſublimated into the neck, and the bladder was expanded. I continued the operation as long as the retort would hold it out. The air collected was likewiſe of the *empyreal kind.*—I mentioned in the ſame memoir a peculiar exploſion, which happened during the diſtillation of zinc with arſenical acid. How natural, how evident is the explication of this phænomenon, after being convinced that here in the retort the *empyreal Air* is in its utmoſt purity, and that zinc is in a red-hot fuſion; and what ſhould be more required for its ignition?

I have often viewed with pleaſure the bright and radiant ſparks, which are produced by heat only in a retort, during the reduction of metallic calxes, when only a little duſt of coals is added to it.

Let us now examine whether this *empyreal Air* is not that kind of Air which had been loſt without Fire, (No. 8,—15.) and even with Fire, (No, 17,—23.)

42.

Experiment I.

I filled a phial of twenty ounce meaſure with *empyreal Air*, by the method deſcribed No. 30, letter *e*. This phial I put inverted in a wide glaſs filled with a ſolution of *hepar ſulphuris*. The ſolution roſe every hour higher into the phial, and in the ſpace of two days the phial was filled with it.

43.

Experiment II.

I mixed in a bottle fourteen parts of that Air which by the liver of ſulphur, No. 8. had loſt its *empyreal Air*, and which, accordingly, (No. 29.) I have called *foul Air*, with four parts of empyreal Air, and put this inverted bottle into a veſſel filled with a ſolution of liver of ſulphur. After the expiration of a fortnight, four parts of empyreal Air were loſt, and the ſolution had riſen into their place.

44.

Experiment III.

Having filled a phial with our Air *(empyreal)* I poured in ſome colourleſs animal oil, and ſtopt it up very carefully; a few hours after it was become brown, and the next day black. It is a difficult taſk for apothecaries to keep this oil white; they are under the neceſſity to divide it into ſmall phials, and to exclude the acceſs of common Air very carefully. If ſuch colourleſs oil be mixed with an acid, though it were diluted with water, theſe acids, together with the oil, grow black in an hour's time. Vinegar has the ſame effect.—There is no other reaſon for the aſſumption of black colour in the Air by this oil, than that the empyreal Air of the common Air deprives this oil of its phlogiſton, and by this means a ſubtle acid is ſet free, which before was united with this phlogiſton, and this produces the black colour.

45.

Experiment IV.

a. I put into a phial of ſeven ounce meaſure, filled with *empyreal Air*, a piece of phoſphorus of urine, and ſhut it up with a a cork; then I heated the place where the phoſphorus lay, by a burning candle; the phoſphorus began to burn with a very vivid flame; as ſoon as the flame was extinguiſhed, the phial burſt in pieces.

b. Since the phial employed in the preceding Experiment, was very thin, I repeated the Experiment with a phial of a thicker ſubſtance, and after it had been left to cool, I wanted to draw the cork under water, but it was not in my power; for the external Air preſſed the cork too hard towards the inſide of the phial. I then puſhed the cork into the phial, immediately after the water ruſhed into the phial, and filled it very nearly full. The firſt phial being very thin, it ſeems the external Air broke the glaſs.

c. Having mixed the *foul Air*, with one third part of the *empyreal* kind, and burnt a piece a piece of phoſphorus in it, only one third of the Air was abſorbed.

46.

Experiment V.

I likewiſe repeated the Experiment deſcribed No. 19, only with this difference, that the tube of it was longer, and that *empyreal Air* was admitted into the matraſs. It was a pretty ſight to obſerve the water mounting gradually into the matraſs, and juſt when 7-8ths of it were filled with water, the flame was extinguiſhed.

47.

Experiment VI.

On the ſtand No. 21, letter *c*. I put a few live coals, and put a matraſs filled with *empyreal Air* over it. The coals had no ſooner reached the Air in the matraſs, than they began to burn with a very vivid fire.

After it had been left for cooling, I made a ſmall aperture under the matraſs, which was filled one-fourth part with water; and having ſeparated from it the aërial acid, by means of the milk of lime, No. 22, only one-fourth remained in the matraſs; and in this reſiduum a candle would burn.

48.

Experiment VII.

I tried likewiſe how *empyreal Air* was affected by ſulphur No. 23. As ſoon as the burning ſulphur came in contact with the empyreal Air in the matraſs, the flame became larger and more vivid. When the fire was extinguiſhed, the water in the veſſel had found its way into the matraſs, and filled 3-4ths of it.—Since I had employed for theſe three laſt Experiments a matraſs containing no more than thirty ounce meaſure of Air, I was obliged to accommodate the ſtand No. 21, to it.

49.

I have mentioned before (No. 16,) that I found the *foul Air* lighter than common Air; ſhould it not follow from thence that *empyreal Air* is heavier than common Air?

In reality, I found, by weighing, moſt exactly, as much *empyreal Air* as occupied the meaſure of twenty ounces of water, that it nearly exceeded the ſame meaſure of common Air by two grains.

50.

Therefore theſe Experiments prove, that *empyreal Air* is that kind of Air by means of which fire burns in common Air; and it is in common Air mixed with a kind of Air, which has not the leaſt attraction of the inflammable; and it is owing to this, that ſome hindrance is made to rapid and violent conflagration; and moſt certainly, if all the Air conſiſted of *empyreal Air*, the water would be of little ſervice for extinguiſhing of fires. The aërial acid, mixed with this *empyreal Air*, has the ſame effect as foul Air. I mixed one part of *empyreal Air* with four parts of aërial acid, in this mixture a candle burnt tolerably. The heat contained in the interſtices of inflammable ſubſtances, cannot poſſibly effect ſo much as the heat in the fire; and I believe myſelf not miſtaken, when I conclude from my own Experiments, that heat is produced and generated during the combuſtion of *empyreal Air*, and the phlogiſton of inflammable ſubſtances; and if this very ſubtle and elaſtic ſubſtance comes in contact with another body attracting the phlogiſton more powerfully, the heat muſt of courſe be again decompounded. The Experiments No. 45, letter *b.* and No. 46. prove this both to the eyes and the feelings, every thing ſeeming entirely to diſappear in the matraſs.

We ſhall now examine, whether the *empyreal Air*, loſt in the Experiments No. 8—15, without Fire, is really converted into heat. The heat is undoubtedly not felt in thoſe caſes, but No. 10. evidently demonſtrates that there is an union of phlogiſton and empyreal Air. It is by no means right to truſt to the ſenſe of feeling, in forming an accurate judgment of the heat; the thermometer is the only ſure guide. If ſulphur burns in a certain quantity of Air about three minutes, and the ſame quantity of Air can withſtand the ſolution of liver of ſulphur ſeveral weeks before all the *empyreal Air* be ſeparated from it it follows, that the heat thus generated, and which however is not one moment without a new addition of it, muſt be very ſlight. And, in a word, we have Experiments wherein Air is abſorbed in half that time, and in that caſe heat muſt be in the double ratio of the former. There are other Experiments where you may feel the heat with your hands; and in thoſe caſes the *empyreal Air* diſappears already within an hour.—I will here communicate the Experiments I made on this ſubject.

51.

Experiment I.

I mixed a ſtrong ſolution of liver of ſulphur, with ſo much of pounded chalk, that it became almoſt a dry powder. Then I put this powder in an open glaſs before the window, and cloſe to it a thermometer: two hours afterwards, both the thermometer and the powder had acquired the ſame degree of heat, when I put the thermometer into the powder contained in the glaſs. Some minutes after this, the ſpirit was riſen: I took

I took the thermometer out, and placed it cloſe by, the ſpirit immediately fell: I again immerſed it into the glaſs, and it roſe again. The next day, I wanted to have the ſame phænomena, but the thermometer having ſtood ſeveral minutes in the powder, the ſpirit did not riſe.—The powder which the preceding day had been yellow, was at preſent white, and by the addition of an acid, no ſmell of liver of ſulphur was produced; conſequently the ſulphur was decompounded; and therefore no wonder that no heat could be produced.

52.

Experiment II.

a. Steel-filings macerated with a few drops of water, cauſe the ſpirit in the thermometer to riſe; which I tried ſeveral times on three conſecutive days, with the ſame ſucceſs, and the ſame filings.

b. Since oil of turpentine abſorbs air, it is probable that here likewiſe heat ſhould be generated; I therefore mixed ſomewhat of this oil with pounded chalk, ſo that it made a looſe powder; and after this mixture and the thermometer were come to a par, I put the thermometer into it, but the ſpirits would neither riſe nor fall.—Since oil of turpentine evaporates very ſtrongly, and all evaporations abſorb the heat of the Air, is it not probable that the ſame happens in this caſe, and that the generated cold be compenſated by the heat which is cauſed? If this does not happen, the ſpirit ſhould fall.

The reaſon for which I mixed the ſolution of liver of ſulphur with oil of turpentine and chalk, is chiefly this; that the Air might come in many more points in contact with the phlogiſton, and cauſe a more ſenſible heat.

53.

Experiment III.

Since only *empyreal Air* cauſes heat with the inflammable principle in our common Air, I was deſirous to know whether the heat generated would not become more ſenſible by employing in theſe Experiments pure *empyreal Air* only. I was convinced of this truth; for having filled a phial of twelve ounce meaſure with *empyreal Air*, and ſhut its mouth carefully up, I let the phial, with a thermometer, a mixture of pounded chalk, and a ſolution of liver of ſulphur, ſtand together, in the ſame place, during the ſpace of four hours; then I put the powder into the phial, the thermometer into the powder, and carefully ſtopped the mouth of the phial round the tube of the thermometer with wax; immediately after the ſpirit roſe to the double of the height which it had riſen to when the thermometer was put into the mixture in the common Air.

54.

Experiment IV.

The heat cauſed by a mixture of iron-filings, ſulphur, and water, muſt be ſolely aſcribed to the union of the phlogiſton in the iron with the *empyreal Air*. I mixed three parts of iron-filings, and one part of ſulphur, with as much water as was required to make of it a moiſt powder; this mixture I divided into two parts; one of them I filled into a phial, and ſhut it up with a well-fitting cork; the other I expoſed to the common Air in an open glaſs; after the ſpace of two hours the laſt mixture was grown ſo intenſely hot that I could not hold the glaſs long in my hand; the firſt was not even grown warm, though both had taken a black colour. Some weeks after I threw part of the ſtill moiſt and ſhut-up powder on a piece of paper; three minutes after, the powder became intenſely heated, and ſmoaked. The reſt of this powder I put in an open glaſs on the ſtand, (No. 21, letter *b.*) and put a matraſs over it. The water roſe gradually in the matraſs, and after three hours about one third of the matraſs was filled with it, and there it remained; then I removed the matraſs, and filled it with freſh Air, and covered with it again the mixture of iron-filings; when the water again roſe.

The water being ſo quick abſorbed, it is no wonder that a ſenſible heat was cauſed; and I flatter myſelf with having proved, that *heat* or *warmth* is compoſed of two ſubſtances, viz. the univerſal inflammable principle, and the empyreal Air contained in common

Air. A man of ſenſe, however, will not immediately infer, wherever heat is generated, theſe two integrant parts of heat muſt at that period form an union;—by no means; ſince heat is likewiſe found in the pores of ſubſtances; whereof we ſhall ſpeak hereafter.

55.

Properties of Heat.

It is well known, that a metal concave mirror will reflect the heat of burning coals put into its focus, in ſuch a manner that when collected by means of another metal concave mirror into a focus, inflammable ſubſtances will be ignited by it. I now aſk, whether the heat of theſe ignited coals, or light alone, or both together, produce this effect? I am well acquainted with the vague phraſes, according to which every thing is called Fire that bears a diſtant ſimilarity to it. In one caſe, light is called Fire; in another, warmth and heat are reputed to be Fire; and ſtill in another the phlogiſton contained in all bodies is miſtaken for Fire. Therefore there will be perſons that will not heſitate to give an anſwer to my query; and ſay: It is Fire which is reflected by theſe mirrors, collected and concentrated; and conſequently the ſame effect is produced as by the light of the ſun. But I am of opinion, it is beſt not to begin to read before one knows the elements of the alphabet; and to withhold an anſwer, till one has reflected on the following Experiments.

56.

a. Sitting in winter in a room oppoſite the ſtove* when the fuel in it is in full blaze, you may ſenſibly feel the heat coming out of the ſtove, when the door is left open at about ſix feet diſtance; however, the breath of the mouth is ſtill viſible, though in ſummer, in a leſs warm atmoſphere, you cannot perceive it.

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* The author, a German Apothecary, being ſettled at Gefla, in Gæſtricke, in Sweden, under about 60°. 40′. north lat. ſpeaks of ſtoves which are uſual in that cold country. They are conſtructed of glazed earthen-ware filled in the inſide with clay and bricks; the fuel is laid on a hearth or grate, and the heated vapours and ſmoke, are obliged to circulate through many ducts managed in the inſide of the ſtove, before they enter the chimney; by which means the heat is communicated to the ſides of the ſtove, and enters the room. The place where the fuel burns may be occaſionally ſhut with an iron door,

b. If you put a burning candle, or cauſe a ſmoke, within the ſphere of the heat iſſuing from the ſtove in ſtrait lines, the candle continues to burn without the leaſt interruption, and the ſmoke riſes perpendicularly.

c. Since there is a conſtant current of Air going from the room into the ſtove, in order to replace the Air rarified by heat and gone off by the chimney, very naturally it will be aſked: Why is the heat, coming from the ſtove into the room, not likewiſe

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door, having a ſmall vent-hole beneath it. The door is commonly open whilſt the wood burns, and all ſmoke and vapours are ſtill going away from the coals to which the wood is reduced. After the coals are quite freed from any noxious vapours, there is a valve at the place where the duct enters the chimney, which, when ſhut, prevents the heat from going off by the chimney. The door to the fire-place is likewiſe ſhut, and theſe precautions preſerve a temperate warmth throughout the whole room.

likewiſe carried off by the current of Air through the pipe or duct of the ſtove?

d. If by any means whatſoever a ſtrong wind is cauſed before and in a direction tranſverſe to the opening of the ſtove, this wind can no more divert the direction of the heat, than that of the beams of the ſun; the face therefore held on the left ſide of the ſtove will be ſenſible of the wind paſſing tranſverſly through the heat, but without any ſenſation of warmth.

e. It is well known, that the ſhade from the light of the ſun with a heated or red-hot body, on an opoſite wall, is ſurrounded by vapours, which ſeem to be tremulous; which are to be accounted for by the greater or leſſer rarefaction of the Air, cauſed by the heat, and the refraction of the beams of light in it: What is then the reaſon, that ſitting oppoſite the ſtove, and having the window on your right hand, and the white wall on your left, nevertheleſs the beams of the ſun coming in by the window, and croſſing the heat before the ſtove in a tranſverſe direction, and falling on the oppoſite white wall, will not repreſent ſuch a tremulous ſhade? But on ſuſpending in this heat a hot iron or ſtone, heated by this very ſame heat, you can immediately obſerve in the open air, and on the wall the uſual tremulous motion.

f. If you hold a large pane of glaſs between your face and the ſtove, you will ſee the Fire, but you have no ſenſation of the heat, and the glaſs receives all the heat.

g. In the ſame manner you may reflect the light of the Fire, with a flat glaſs mirror, and however not feel the leaſt heat in the reflected light; whereas the looking-glaſs will receive and keep all the heat falling on it.

h. But a poliſhed metal plate will reflect both the light and the heat according to the laws of the light of the ſun; and ſince the heat is reflected, it is no wonder that the metal plate is by no means warm.

i. For the ſame reaſon, you may with a ſmall metal concave mirror before this ſtove at the diſtance of four feet, form a focus, wherein you may alight ſulphur. In this ſituation you may keep the mirror very long, and it will not grow warm; but if covered with black over a burning candle, you cannot keep it four minutes in the ſame ſituation without burning your fingers.

k. If you reflect the heat paſſing out of the ſtove with a poliſhed metal plate to another ſpot, you may, at the diſtance only of four or ſix feet, form a focus with it which has ſenſible heat; but if you collect, with a concave mirror, the light reflected by a glaſs mirror, the focus will be very bright, but deſtitute of all heat.

l. If you hold a glaſs pane between yourſelf and the ſtove, you may form a bright focus with the concave mirror, which however has no heat. For the ſame reaſon you may with a convex glaſs lens, form a bright focus before this fire, which, however, is void of all heat.

m. The metal concave mirror, and the metal plate grow ſoon hot, if they come in contact with a heated body; though the heat paſſing out of the ſtove will not impart heat to them. If the tube or duct carrying the ſmoke from the ſtove into the chimney be ſhut, the heated Air immediately paſſes through the door of the ſtove in a perpendicular direction upwards. If you preſent a metal concave mirror, or metal plate to this heat riſing perpendicularly, you may not only reflect the heat, but the metals themſelves will be heated.

57.

From theſe Experiments it appears, that the heat mounting with the Air in the ſtove, and paſſing through the chimney, is materially different from the heat paſſing through the door of the ſtove into the room: That the heat departs from the centre, where it is generated in ſtrait lines, and is reflected from poliſhed metals under an angle which is equal to that of incidence (No. 56. letters *b. i.*): That it unites not with Air, nor can be diverted by a current of Air, into a direction different from that which it originally had received (letters *c. d.*) For that very reaſon the vapours of the breath are viſible in this great heat (letter *a.*); for ſince Air and Heat are really united during ſummer, and warm Air can diſſolve more water than cold Air, it likewiſe hence appears, that this kind of heat is not united with Air; nor has this kind of heat probably rarefied the Air; and of conſequence it becomes evident, why it cauſes no tremulous motion in ſunſhine (letter *e*). Theſe are properties of light; I will not, however, believe, that on account of it theſe phænomena will be aſcribed to the light emanating from the flame; for, in the firſt place, this light is, in regard to that of the ſun, very weak; and ſecondly, the above-mentioned effect of conflagration is (letter *i.*) infinitely more ſtrong when the wood is conſumed and changed into bright, glowing coals, at which time the light is much decreaſed; and you may, by means of a glaſs mirror, (letter *g.*) ſeparate the light from this heat, when the heat remains in the glaſs, and the radiant light yields no heat, which appears by letters *g. l.* Hence it follows, that the heat paſſing through the door of the ſtove coincides, in ſome points, with the light, but is not yet quite become light, ſince it is not reflected in the ſame manner from a glaſs ſurface as from a metallic one—*a remarkable circumſtance!* It becomes likewiſe active only at a ſmaller diſtance from the point where it is generated, at leaſt ſenſibly ſo; but it is ſoon changed into the uſual heat, whenever it unites with a body; which may be obſerved in the glaſs *(g.)* and in the metallic concave mirror blackened by ſoot *(i.)* and in more inſtances; whence heat may be tranſferred from one body into another, and, in conſequence, unite with air, and produce the tremulous motion *(c)*. This is applicable not only to the heat paſſing from the ſtove into the room, but alſo to any fire whatſoever. Repreſent to yourſelf a little hillock of burning coals; in this caſe the heat darting from this hillock all around, is that which may be reflected by a metallic poliſhed plate; that on the contrary which riſes upwards, and may be driven by winds to and fro, unites with Air. I will call the firſt kind, for diſtinction ſake, *radiant heat*.

58.

Since theſe are two diſtinct kinds of heat, which are poſſeſſed of ſo conſiderable a difference, it occaſions the following queries: Wherein conſiſts this difference? Did it, in the moment of its generation, not meet with a ſufficient maſs of Air, with which it might enter into an union of coheſion? or ſhould it, at its firſt exiſtence, have received ſo great an elaſticity, that in its ſwift motion it can neither by Air nor by poliſhed metals be detained? The firſt opinion has probably no great weight; for it will ſoon be objected, why does it not unite afterwards with Air, but paſſes like beams of light through it? The laſt ſeems to me, for certain reaſons, to be very probable. However, which is it that communicates to heat ſo great a degree of elaſticity? I imagine *empyreal Air* is capable of being united with either more or leſs phlogiſton: Then, undoubtedly, *empyreal Air* ought to preſent us with phænomena proportionate to the quantity of phlogiſton united with it. Do we not obſerve, that many bodies admitting of an union with phlogiſton, are capable of receiving either a larger or ſmaller proportion of it? and do they not, by this means, obtain either a greater or leſſer degree of volatility and elaſticity, as mentioned before, (No. 27.) and is demonſtrable from the ſpirit of nitre? *Empyreal Air* ought to be poſſeſſed of the ſame property, ſince, together with phlogiſton, they are conſtituent parts of heat; and I ſhall, in the ſequel, prove that heat is capable of uniting with more phlogiſton. The encreaſed elaſticity of heat communicated to it by phlogiſton, is diminiſhed by the attraction of bodies, ſo that both poliſhed metals and Air may attract it.—Now it will be eaſy to anſwer to the query propoſed, No. 55. For it is *radiant heat*, which is inviſible and diſtinct from fire, that cauſes the ignition there mentioned.

59.

About Light.

Hitherto I have, as much as was required for my intended definition of Fire, ſhewn, from the moſt incontrovertible Experiments, what are the conſtituent parts of heat, and likewiſe thoſe of Air. But ſince there is no Fire without light, this wonderful phænomenon remains to be explained before fundamental inſtruction can be given about Fire and its conſtituent parts.

There is no doubt about the light of the ſun, and that of a burning candle being the ſame thing; for this affects the eye in the ſame manner as the ſun, and repreſents the ſame colours through the priſm; but being weaker, it is no wonder, that its beams, collected by a burning glaſs, will not burn.

Nor is there any more doubt about light being a body, in the ſame manner as heat: but I cannot perſuade myſelf that light and heat are the ſame thing, ſince Experiments prove juſt the contrary; what is now to be ſaid ſhall explain the ſubject more diſtinctly.

60.

Proofs of the Exiſtence of an Inflammable Principle in Light.

It is well known, that the ſolution of ſilver in acid of nitre poured on a piece of chalk, and expoſed to the beams of the ſun, grows black. The light of the ſun reflected from a white wall has the ſame effect, though more ſlowly. Heat without light has no effect on this mixture. Should the black colour not be real ſilver? We ſhall not decide this queſtion until I have given the proof of the phlogiſton being found in light.

61.

Experiment I.

I put ſome ſilver-earth on a piece of porcelane, and expoſed it to the focus of a burning-glaſs; immediately the ſurface of this earth was reduced to ſilver. By ſilver-earth, I underſtand ſilver diſſolved in pure acid of nitre, and precipitated by ſalt of tartar. The acid of nitre moſt certainly deprives the noble and baſe metals of their phlogiſton (No. 27. letter *b.*) which is evidently proved from the effervеſcence cauſed during the ſolution, and likewiſe from the *red* colour of the vapours. Metallic precipitates are diſſolved in acid of nitre, but communicate to it not the leaſt red colour, the ſame takes place in regard to the earth of ſilver. The ſilver reduced in the focus, communicates to acid of nitre, during its ſolution a redneſs; and whence could this phlogiſton poſſibly come, but from the light of the ſun?

62.

Experiment II.

a. I put on a ducat ſome calx of mercury, prepaired by acid of nitre, or what is commonly called *red precipitate*, and expoſed it to the focus of a burning-glaſs. The powder began to emit fumes, and the gold became white.

b. I diſſolved gold in aqua regis, made of aquafort and common ſalt, and precipitated the ſame with ſalt of tartar. This gold-earth, edulcorated and dried, was expoſed to the focus on a piece of porcelane; it became brown, and had the properties of real gold.

c. This reduction might be aſcribed to the heat which is actually exiſting in the focus. Suppoſing, therefore, the heat to be the cauſe of this phænomenon, then even the inflammable principle is demonſtrated to exiſt in light; ſince no heat can exiſt without phlogiſton; but there are many more arguments contrary to this opinion.

d. I poured a ſmall quantity of the pureſt fuming ſpirit of nitre (No. 25) in a white phial made of cryſtal glaſs provided with a glaſs ſtopper, and expoſed it to the light of the ſun. Three hours after I found the phial filled with red vapours; the ſame happens when the phial is ſet on a German earthen-ware ſtove; but it requires four weeks before the red colour becomes diſcernible.

63.

Experiment III.

a. I precipitated a ſolution of ſilver by ſal ammoniac; then I edulcorated and dried the precipitate, and expoſed it on a piece of paper to the beams of the ſun for the ſpace of two weeks, when the ſurface of the white powder grew black; after which I ſtirred the powder, and repeated the ſame ſeveral times. Hereupon I poured ſome cauſtic ſpirit of ſal ammoniac on this, in all appearance, black powder, and ſet it by for digeſtion. This menſtruum diſſolved a quantity of *luna cornua* (horn-ſilver) though ſome black powder remained undiſſolved. The powder having been waſhed, was, for the greater part, diſſolved by a pure acid of nitre, which, by the operation, acquired volatility. This ſolution I precipitated again by means of ſal ammoniac into horn-ſilver.—Hence it follows, that the blackneſs which the luna cornua acquires from the ſun's light, and likewiſe the ſolution of ſilver poured on chalk (No. 60) is ſilver by reduction.—I kept the white horn-ſilver wrapped up in paper during the ſpace of two months, on a warm earthen-ware ſtove, which did not in the leaſt alter its colour. But ſince ſilver never can unite with marine acid under its metallic form, it follows, that as many particles of luna cornua are reduced on its ſurface into ſilver, as many likewiſe ought to part with their marine acid, with which they were united.

b. To verify this, I mixed ſo much of diſtilled water with well edulcorated horn-ſilver as would juſt cover this powder. The half of this mixture I poured into a white cryſtal phial, expoſed it to the beams of the ſun, and ſhook it ſeveral times each day; the other half I ſet by in a dark place. After having expoſed the one mixture, during the ſpace of two weeks, I filtrated the water, ſtanding over the luna cornua, grown already black; I let ſome of this water fall by drops in a ſolution of ſilver, which was immediately precipitated into horn ſilver. The water which had been ſtanding over the other half of the luna cornua, in a dark place, did not change the ſolution of ſilver; and the horn-ſilver, over which the water had been ſtanding, had not in the leaſt changed its white colour.

c. I poured aquafortis upon horn-ſilver, and expoſed it in a cryſtal phial to the beams of the ſun; but it did not grow black; the cauſe of which appears from No. 62, letter *c*.

64.

Experiment IV.

I evaporated a ſolution of gold till it was dry; then I diſſolved the *reſiduum* in diſtilled water, poured it into a white cryſtal phial, ſhut up with a glaſs ſtopper, and expoſed it to the beams of the ſun. A fortnight after, I obſerved (eſpecially when I examined the ſolution in ſunſhine) that a quantity of minute ſpangles of gold appeared in the ſolution, and the ſurface was likewiſe covered with a very ſubtle pellicle of gold.—The evaporation of the ſolution of gold, was done with a view to free it from the ſuperfluous acid, which in a certain meaſure would have hindered the reduction.—Now I am willing to mention another Experiment, which muſt convince us ſtill more of the preſence of a phlogiſton in light. A pure aquafortis diſſolves not *manganeſe* (Molybdænum, Magneſia, Linn. Manganeſe, Cronſtedt's Mineralogy, ſect. cxiii.) unleſs ſome inflammable ſubſtance, viz. ſugar, be added; when the ſolution becomes clear like water, and without any colour. If ſuch a ſolution be precipitated by ſalt of tartar, a white precipitate is yielded; which after being edulcorated and dried, is nothing elſe but manganeſe united with the phlogiſton of ſugar. If you ſeparate the phlogiſton in any manner whatſoever, you have again the manganeſe in its natural black colour. (See the Memoirs of the Swediſh Academy for the year 1774). To effectuate this, there is no ſhorter method than that of ſpreading the phlogiſticated manganeſe on a thin metal plate, and expoſing it to the heat of burning coals, when it ſoon acquires again its black colour. This manganeſe, be it ever ſo finely pounded, never diſſolves in acid of nitre without an addition of phlogiſton; and of this I am going to ſpeak in the ſequel.

65.

Experiment V.

I poured about half an ounce of pure and fuming ſpirit of nitre, in a cryſtal phial, of which 7-8ths remained empty; into this I put ſome of the before-mentioned manganeſe, ſhut the phial with a glaſs ſtopper, and expoſed the phial during two hours to ſun-ſhine. During this time the mixture had loſt its black colour, and was become clear. Then I added ſomewhat of the fine pounded manganeſe; and having ſtopped up the phial, expoſed it again to the beams of the ſun; ſome hours after this the manganeſe was likewiſe diſſolved. I repeated this operation till the acid would no more diſſolve the manganeſe. Then I added to it ſix times its bulk of diſtilled water, and filtrated the ſolution, which I afterwards precipitated by ſalt of tartar: The white precipitate I waſhed carefully, and dried it in a moderate degree of warmth. This now is the manganeſe united with phlogiſton, with an addition of *aërial acid* from the alkali.—If you want to be convinced of it, diſſolve it in vitriolic acid, and diſtill it by a ſtrong fire in a ſmall glaſs retort. The manganeſe remaining in the retort has then recovered its natural colour; and the acid gone over, has acquired all the qualities of a volatile ſpirit of ſulphur.—Or take one part of pounded ſaltpetre, and mix it with four parts of this white manganeſe; put the mixture into a ſmall retort and diſtill it: the mixture ſoon acquires a black colour, and the ſaltpetre is found to be alkalized.—Or, ſtill better: Fill a ſmall phial with this phlogiſticated manganeſe, fit a ſtopper of chalk into its mouth, ſurround the phial with ſand in a crucible, and then keep the phial for about a quarter of an hour red-hot; whilſt it is ſtill tolerably hot, take it out of the ſand and throw the ſtill white manganeſe on a paper, and it will ſoon after deflagrate, and the manganeſe turn into a black powder: Or let it grow quite cold, throw it then on a hot (though not red-hot) metal plate, when the manganeſe will become red-hot, and be reſtored to its original nature. If you aſk, why the manganeſe ought to be made red-hot in a cloſe veſſel, in order to bring about this deflagration, I anſwer, if ſuch white manganeſe, before it be long uſtulated in a cloſe veſſel, be put on a hot iron plate, it is likewiſe calcined, but the deflagration is ſcarcely viſible, on account of the aërial acid with which it is united, for this is carried off into the Air in proportion as the phlogiſton flies off. Now one part of aërial acid mixed with eight or ten parts of common Air (No. 22) extinguiſhes Fire; it is therefore neceſſary to expel firſt the aërial acid in cloſe veſſels, and after this operation the phlogiſton ſeparated from the beams of the ſun really deflagrates.

66.

Light is not a Primitive or Elementary Subſtance.

If light were a primitive ſubſtance nobody would heſitate to conclude, from the juſt mentioned Experiments, and others known before, that it cannot be any thing but the inflammable principle or phlogiſton. But having myſelf proved, that this elementary ſubſtance, in conjunction with *empyreal Air*, compounds the heat or warmth, and that our atmoſphere is filled with a great quantity of empyreal Air, it would then follow, that the phlogiſton conſtantly flowing out from the ſun, unites with our empyreal Air, and cauſes nothing but heat; and that we in conſequence muſt dwell in utter darkneſs. We find, however, that light, though ever ſo much concentrated, cannot produce any heat in the Air; conſequently I cannot perſuade myſelf that light is pure phlogiſton.—*Secondly*, I cannot collect from my own Experiments, that light ſhould be nothing but phlogiſton; for in that caſe it would alkalize ſaltpetre in the focus of a burning-glaſs, and bring about the reduction of the calxes of the baſe metals, which never happens.—It may be objected to me, that as much as there is reduced of theſe calxes, as much is calcined by the Air, through the heat in the focus. It is therefore chiefly neceſſary to make this Experiment in an Air incapable of receiving any more phlogiſton, *i. e.* in foul Air. Mr. *Lavoiſier*, and others, have already obviated this objection. He calcined metals under a glaſs receiver by means of a burning-glaſs, and they were deprived of their phlogiſton only in a certain quantity of Air; *i. e.* in proportion to the *empyreal Air* contained under the receiver ſo much and no more phlogiſton could be ſeparated from the metals; the Air remaining under the receiver, being foul Air. Why could Mr. *Lavoiſier* not reduce again this calx in this ſame Air, after having continued the calcination, if the objection had any weight, ſince this Air cannot receive any more of phlogiſton?—Again, it might be objected; if light were not a *ſubtle*, a *pure* phlogiſton, it would reduce the calxes of baſe metals as well as thoſe of the noble ones, ſince the baſe metals have in their mixture only a *coarſe* phlogiſton. I anſwer, the phlogiſton is in all ſubſtances the ſame, and not in the leaſt different, that exiſting in gold and in ſilver is ſimilar to that found in iron and oil. The baſer metals reduce the noble ones, for inſtance, copper reſtores the metallic earth, or calx of ſilver, or quickſilver, diſſolved in ſpirit of nitre, again to its metallic form.—It may again be ſaid, that the phlogiſton of copper is decompounded, and only its more ſubtle particles (ſuch as are ſimilar to thoſe which penetrate through the retort, and reduce the calxes of the nobler metals) are communicated to the ſilver earth.—Then I aſk, What is the remaining part of the coarſe phlogiſton, which is ſtill left in the ſolution of copper? If any ſolid ſubſtance ſhould be ſeparated from the ſubtle phlogiſton which was united to the ſilver earth, (Mr. *Beaumé* thinks it is a ſiliceous earth) it would follow, that the copper ſolution in this ſtate (after having been evaporated till it be dry, and then ſtrongly

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ſtrongly diſtilled in a retort, by which means the acid of nitre will be again ſeparated) ſhould have its calx of copper left in the *reſiduum* again reduced into copper by a mere red-hot heat; ſince the ſubtle earth wherewith the pure phlogiſton is imagined to have before been united, is ſtill preſent, ſo that the pure phlogiſton penetrating through the crucible could again be reſtored to it: But experience has proved to me that this cannot be done.—It appears evidently, that all ſuch opinions have their origin in the ignorance we hitherto have been in with regard to the eſſential property of heat, *which has been looked upon as a ſubtle phlogiſton.*

The beautiful colours, wherewith light always ſhines, are, *thirdly*, likewiſe proofs that the light is not ſolely a phlogiſton. The attraction with which they both act ſo differently upon the different bodies, ſufficiently proves that they cannot be analogous; and the following Experiment gives to this opinion ſtill more weight: Fix a glaſs priſm at the window, and let the refracted ſun-beams fall on the floor; in this coloured light put a paper ſtrewed with luna cornua, and you will obſerve that this horn-ſilver grows ſooner black in the violet ray than in any of the other rays: *i. e.* that the calx of ſilver ſeparates the phlogiſton ſooner from the violet ray of light than from any other colour.—Having demonſtrated the exiſtence of an inflammable principle in the light, and having beſides proved that light conſiſts not ſolely of phlogiſton, it may be inferred, that light can by no means be conſidered as a primitive ſubſtance.

67.

Light, if its motion be not interrupted, cauſes neither Heat nor Cold.

If you take two thermometers which are exactly equal, and one of which is filled with deep red ſpirits, and the other with an uncoloured ſpirit, and expoſe them both to the beams of the ſun, you will obſerve that the coloured ſpirits riſe ſooner than the uncoloured ones; but immerſing both in the ſame hot water, they riſe both with equal celerity. The more a body approaches in colour to black, the ſooner will it be heated by the beams of the ſun; the whiter it is, the later it will be heated: However, if both be immerſed in a warm fluid, they both acquire the ſame degree of heat. The cauſe of the difference of theſe phænomena is to be found in the greater or leſſer attraction the bodies have to light. For this very reaſon, the bodies that reflect the incident light in all poſſible directions, and are called *white*, grow little and ſlowly warm: the ſame may be ſaid of bodies which let the ſun-beams paſs without hindrance, and are commonly called tranſparent bodies. Heat cauſed by the rays of the ſun, is owing to their velocity being impeded by certain bodies. Here ariſes the queſtion: Whether this heat is *actually a property of the rays of the ſun, or whether it originates from the bodies?*

68.

I have, in ſome of the preceding ſections, mentioned Experiments by which it appears, that the *radiant heat* (No. 57.) is attracted neither by Air nor poliſhed metals; and that by the means of a metallic concave mirror, it produces a burning focus;

and likewiſe, that after having been brought into union with other bodies, it eaſily unites with the Air and metals: Theſe are properties which are alſo belonging to light. I then likewiſe proved, that the igniting power of *radiant heat* is not to be aſcribed to the light mixed with it, (No. 27.) but that it then only produces ſuch effects, when this heat has been attracted by bodies: And are theſe not likewiſe properties of the beams of the ſun?—I will ſuppoſe, the heat which to the greater part of bodies is communicated by the light of the ſun, is the ſame which is contained in the pores of bodies; but, ſince it is thought that all heat ariſes from friction, that it is ſet in motion by the friction cauſed by the light of the ſun. Air, which in its pure ſtate is not ſenſibly heated by the rays of the ſun, is however heated when it ſurrounds a body, upon which the light of the ſun has operated for a ſhort while: This is the chief cauſe of the heat we obſerve in the air during ſummer. This ſuppoſed, it would follow, that the body muſt part with ſome of its natural quantity of heat; if this happens, it muſt make a conſiderable quantity, when the ſun has been ſhining on the body almoſt every day during a whole ſummer. Nevertheleſs I obſerved that, for inſtance, a piece of iron becomes, in autumn, from the light of the ſun, to my feeling, as hot as in ſpring, or when it is bent backwards and forwards. A very weighty objection may be made here; viz. that the heat which the body illuminated by the ſun-beams loſes during day, may again be attracted after the ſetting of the ſun, and during night, from the Air and thoſe bodies which ſurround it; and that this perhaps is one of the cauſes why it grows ſo cool in the evenings and during night. In order therefore to obviate this difficulty, I made the following Experiment, June the 22d; I hung a piece of ſheet lead coloured black, in the open Air, ſo that it might be expoſed to the ſun during the whole day; the lead was conſtantly ſo intenſely hot that a tender hand could not hold it for a long time. Two hours before ſun-ſetting, I rolled the lead up and immerſed it in a beer-glaſs filled with water; cloſe to it I had put another beer-glaſs likewiſe filled with water, into both I had immerſed thermometers. Immediately after, the ſpirits of the thermometer in the glaſs where lead had been immerſed began ſomething to riſe, on account of the ſheet-lead. After the ſpace of two hours, the heat was equal in both glaſſes. I continued to obſerve the height of the thermometers during all the night, but as much as the one fell, the other fell likewiſe. Should the water wherein the lead had been immerſed not have loſt a greater quantity of heat, if the above objection were founded?--I am therefore apt to believe, that the light of the ſun imparts no heat whenever it continues its ſtraight motion uninterrupted; but when it is retarded by the attracting powers of bodies, its heat becomes immediately ſenſible;—So likewiſe it happens with the *radiant heat*, (No. 57.)

69.

Conſtituent Parts of Light.

Since the beams of light falling on a body which has an attraction to them are changed into heat, it might at firſt outſet ſeem, that light is nothing elſe but heat moved with an incredible velocity:

for it extends bodies to which it is united in the ſame manner as heat does. Our nerves receive from this heat the ſame motion which is communicated to them from the heat of Fire; and it is likewiſe inviſible, juſt as heat is. But it ought not then to cauſe the reduction of the precipitate gold; it would not make *luna cornua* black; the fuming ſpirit of nitre would not be tinged red; nor would manganeſe be diſſolved in it; which is juſt the contrary of what I have proved, (No. 62, 63—65.) This may be obſerved by covering the phial containing any one of the above mixtures with a thick coating of black paint, and then expoſing it for ſeveral days together to the operation of the ſun-beams; the phial grows very hot, but the ſubſtances contained do not undergo the leaſt change. Light, therefore, intercepted by certain bodies, produces the phænomena of heat. It is therefore more than probable, that light is compoſed of the ſame principles as heat.—However, ſince light, when it moves on in a free and undiſturbed direction, is poſſeſſed of ſtill more properties than thoſe belonging to heat or *radiant heat*, (No. 57,) it may be hence inferred, that it is not to be conſidered as heat only; at leaſt it entitles one to the belief, that its integrant parts are made up in a proportion entirely different from that of heat. To thoſe who occupy themſelves with chemiſtry in a rational manner, it cannot be unknown, that there are a great many ſubſtances which unite with the inflammable principle, either in a greater or leſſer proportion, and in each of theſe unions acquire different properties. I have ſpoken already of it (No. 27,) and the acid of vitrol proves the ſame thing: Why then ſhould this not take place in regard to heat, ſince I have proved, that it is compoſed of phlogiſton and *empyreal Air*? If this *empyreal Air* is compoſed with more phlogiſton than is neceſſary for producing heat, *radiant heat* is produced; if ſome very little phlogiſton be added, the property diſcovered in radiant heat is then increaſed and *light* is produced. Nay, the leaſt atom more or leſs of phlogiſton, produces the moſt glorious phænomenon of the various colours, that is to ſay, the various kinds of light. Among theſe the violet and purple coloured light is charged with the leaſt quantity of phlogiſton, ſince it is more attracted by the priſm than all the other ſpecies of light; eſpecially obſerving, that radiant heat (No. 57,) which undoubtedly is charged with ſomewhat leſs phlogiſton than light, is likewiſe and ſtill more ſo attracted by the priſm. Whence it appears, why the eye can bear a longer time to dwell upon the violet rays than the red ones, viz. becauſe each particle of light in the red ray is charged with ſomewhat more phlogiſton; for which reaſon this light, be it ever ſo ſubtle, muſt conſiſt of greater particles than the violet colour, and therefore produce a greater effect on the nerves of viſion. The violet beams produce the reduction of horn-ſilver ſooner than the reſt (No.66). It ſeems that ſince theſe beams are more ſtrongly attracted by the priſm than all the reſt, they move afterwards with a velocity ſomewhat diminiſhed; whence the calx of ſilver can keep up its attraction the longer, and conſequently decompound the violet light.

I therefore am of opinion that each particle of *Light* is nothing more than a *ſubtle particle of empyreal Air, which is more charged*

charged with phlogiſton than an equally ſubtle particle of heat.

70.

The experiments mentioned in the beginning of the preceding ſection, give an opportunity of putting the queſtion, Why light produces quite different properties after it has been attracted by bodies, and thence acquired the chief properties of heat?

It would be eaſy to anſwer, if I could prove that all bodies retained the ſuperfluous phlogiſton of light; but this is done by few only; and if this were the caſe, all bodies ſtruck during ſome time by ſun beams muſt have undergone ſenſible alterations, which however has not been obſerved. —It is equally difficult to account for the change of *radiant heat* (No. 57.) into heat, by the attracting bodies.—The chemical affinities producing the ſtrangeſt changes in bodies, become only then active when the bodies come in every poſſible point of contact with one another. Heat, as a matter, cannot penetrate a body, and it occupies only its pores; in which caſe only a few points of its matter come in contact with it: if more heat be added, it comes in contact with ſtill more points; if ſtill more heat be as it were forced into it, and the body is of ſuch a nature that it attracts the inflammable principle ſtronger than the empyreal Air does, heat is deſtroyed. This is the caſe with the reduction of the nobler metallic calxes by heat only, which never happens before theſe calxes either acquire a red heat, or at leaſt nearly approach to that heat.—If I apply this theory to light, this ſubtle and with incredible velocity moving ſubſtance, muſt exactly touch the bodies; and by thoſe poſſeſſed of attraction to the phlogiſton, be in the ſame moment decompounded: But after that its violent motion has been ſtopped by the attracting powers of other bodies, and it ſhews its activity only as mere heat, occupies the pores of theſe bodies, paſſes from them into others containing leſs of it, without touching much or at all the matter of theſe bodies; and in conſequence the *luna cornua*, &c. &c. cannot be reduced.

71.

On Fire.

At laſt I come to my chief ſubject, which properly has been the object of all the preceding Experiments.—I know how very uſeful the thorough knowledge of *Fire* muſt be to him who is deſirous of obtaining a perfect knowledge of the properties and conſtituent parts of all bodies. Inquiries comprehending the principles and explications of ſo many other phænomena, muſt be made with great care and accuracy, in order to avoid all falſe ſyſtems, which lead us from one uncertainty into another: Inquiries which thoſe chiefly ought to take upon themſelves, and become anſwerable for, who make ſo many if not the greateſt part of their Experiments in fire: Chemiſts are thoſe whom I mean. But how is it poſſible to obtain a clear inſight into this matter, when people are diſcouraged both by written and verbal accounts of the great difficulties obvious in the inveſtigation of fire, which amount to this; that there are many centuries elapſed, and neverthelеſs ſo very little certainty has been brought to light relative to its properties? But far be it, that he who is deſirous of acquiring the knowledge of natural phænomena, ſhould take this for a rule of his conduct, and give up the hopes of obtaining more certainty and inſight in this affair.—Others, on the contrary, explain Fire ſo eaſily, that by their account it ſhould ſeem to become palpable. They do not conſider the many objections to which their doctrine is liable: According to the one, heat is fire; and to another, light is Fire: Here it is aſſerted that heat is the elementary Fire; and there again, it is the effect of Fire. By the principles of one, light is the pureſt Fire, and an element; and by thoſe of another, light is diſſeminated through the whole ſpace of our world, and by the impulſe of the elementary Fire, it is put into a rectilinear motion. Here you are given to underſtand that light is an element, which is confined by means of the *acidum pingue*,* and by the expanſion of this imaginary acid it is again ſet at liberty, &c. What is therefore more neceſſary than to make new Experiments, in order to get out of this labyrinth ?—But before I enter upon the ſubject I am obliged to explain what I underſtand by the word *phlogiſton.*

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* By Mr. Meyer; who uſed that expreſſion in a treatiſe on lime, full of very curious Experiments.

72.

Phlogiſton.

1. *Phlogiſton* is a true element and a ſimple principle.* 2. It can, by the reten-

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* There are a great many who believe that phlogiſton is a compound of elementary fire (by which they mean heat) and of a ſubtle earth. This earth is, according to the opinion of Mr. Beaumé, the ſiliceous, which he thinks to be the primitive earth. When this earth conflagrates, heat is ſeparated from it, and goes into the earth. Is this elementary fire united with Air, or is it only diſſipated in it ? Why is it not poſſible to compound phlogiſton of heat and ſiliceous earth ? Mr. Beaumé ſays, the coal-like reſiduum of diſtilled oils is almoſt pure phlogiſton. If ſuch a ſubtle coal is burnt, but very little earth remains. It is incomprehenſible how ſo ſmall a quantity of earth, could abſorb ſo great a quantity of heat or elementary

tive power of certain matters, be transferred from one body into the other; theſe bodies then undergoing material changes, ſo that not ſeldom they are enabled, by the effect of the particles inſinuating themſelves in the interſtices of bodies, to go over into fuſion, or even into elaſtic vapours; and in this ſtate it is the chief cauſe of ſmelling. 3. Very often it brings the particles of bodies into ſuch a ſituation that they attract either all the beams of light, or only ſome one, or even none. 4. In the tranſition from one body into another neither light nor heat is communicated to it. 5. This element enters into ſo cloſe and ſubtle an union with *empyreal Air* that it even penetrates through the moſt ſubtle pores of all bodies. From this union ariſes both the matter of light, and likewiſe the matter of heat. In all theſe compoſitions phlogiſton undergoes not the leaſt change; and from the laſt compoſition it can be ſeparated again. Phlogiſton can by no means be obtained

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mentary fire; for what, after conflagration is wanting, is the weight of heat. But the aërial acid which is ſeparated from this coal in ſuch great quantities during conflagration weighs it nothing? *The Author.*

obtained by itſelf; for it never ſeparates from one body, though united with it ever ſo cloſely, unleſs there be preſent another coming in immediate contact with it.

73.

Combuſtible Bodies.

Thoſe bodies commonly called combuſtible, are either hard, or ſoft, or fluid. Sulphur, pit-coals, zinc, yellow amber, wax, camphire, oil, ſpirit of wine, &c. belong to them. Phlogiſton exiſts in them in great quantity, but not ſtrongly united with them.—A number of Experiments ſeem to prove, that the acid principle is properly the ſubſtance with which phlogiſton is capable of forming a more or leſs firm union.—The objection made to this aſſertion relative to metallic earths ſeems to me to be of no conſequence. I ſee that the acid of arſenic, with a ſmall portion of inflammable, acquires the appearance of an earth, and with ſtill more of it the form of a regulus (No. 41). How hard were it to ſuppoſe, that all metallic earths, nay, all kinds of earth, are different kinds of acid?-- Water is the chief earth made fluid by heat. This ſubſtance communicates fixity to acids, though both be volatile. Phoſphoric acid is volatile: this may be ſeen when phoſphorus is burnt in a cloſe phial, the acid attaches itſelf every where to the phial, and may be ſublimed by a burning candle from one ſide of the phial to the other; but by adding water, this acid will bear red heat without evaporating. Fuming acid of vitriol, fuming acid of nitre, and fuming marine acid, nay, even concentrated vinegar, may every one be ſomewhat fixed by water. Of thoſe acids which properly are thus fixed by watery vapours, ſo that they can be called earths, we hitherto know but one; which is the acid of ſparry fluor, yielding with watery vapours the ſiliceous earth; an earth whoſe conſtituent parts chemiſtry has not yet been able again to decompound. —The properties of metallic earths in attracting the phlogiſton, muſt depend upon the nature of their acids: the vitriolic, nitrous, and phoſphoric acids attract it very ſtrongly, the marine and ſparry fluor acid attract it but weakly; for which reaſon the ſiliceous earth is found to have no affinity with it. The union of moſt earths with acids, depends merely on a ſmall portion of phlogiſton wherewith the acids or earths are intimately united. It is manganeſe which gives me an opportunity of ſupecting this: manganeſe has a great ſimilarity with the ſiliceous earth, it is likewiſe indiſſoluble in the acids; but if a phlogiſton be added to its mixture, it acquires all the properties of an abſorbent earth (No. 64). If it were poſſible to ſeparate, in a rational manner, the phlogiſton ſo firmly united with metallic and other abſorbent earths, they would probably diſcover their acid nature more palpably.---Who ſees not, that here is opened a wide field for new and beautiful Experiments? But I find that I am deviating too far from my intent.

74.

We are pretty well acquainted with *oily mixtures:* we know the conſtituent parts of ſulphur and phoſphorus. Although the imitation of animal and vegetable oils is very difficult, we know, however, their conſtituent parts; and after a mature conſideration it will become obvious, that it is a difficult taſk to compoſe them by chemical operations.—We find, in a moſt ſcrupulous and total analyſis of ſuch oils, nothing but phlogiſton, aërial acid, and water. It is believed, that they beſides contain an acid ſomewhat ſimilar to vinegar, which may be actually ſeparated from them in a ſmall proportion by diſtillation: However, ſince this acid is again to be analyzed, and then contains water, aërial acid, and phlogiſton; and it is probable that the above acid is generated from the above conſtituent parts during diſtillation; and beſides, none has been able to compoſe of vegetable acids and phlogiſton an oil; I am apt to believe, that ſuch decompounded acid may be conſidered as a conſtituent part of oils: But why are we not able to compound ſuch oils, as well as we can compound ſulphur? When the phlogiſton is to be united with aërial acid, ſuch a body is to be employed as contains phlogiſton: If you ſhould employ for that purpoſe ſuch a body as has a ſtronger affinity to phlogiſton than the aërial acid, you would act very improperly. The phoſphorus, vitriolic, nitrous acids, metallic earths, and *empyreal Air*, are ſuch as decompound coals and oil; and in theſe laſt, the phlogiſton is united with aërial acid. The greater or leſſer quantity of earth contained in theſe oily ſubſtances is only to be conſidered as accidental; for as little as earth is requiſite for making ſulphur and phoſphorus, as little is earth neceſſary in this caſe. How difficult muſt this compoſition be!—Thus much is true, their conſtituent parts are phlogiſton, aërial acid, and water.

75.

Fire.

Fire is that more or leſs heating, and more or leſs luminous *ſtate* of certain bodies, into which they come by means of the Air having previouſly been heated to a certain degree; in which ſtate they are reſolved into their conſtituent parts, and entirely deſtroyed, whereby conſtantly a certain part of the Air is loſt.

Remark 1. Hence it likewiſe appears, that the heating of ſtones, earths, ſalts, &c.

with a red heat cannot be called a Fire; ſince the air undergoes by it no change, its expanſion excepted, and ſince the operation can be done even without Air.

Remark 2. Heat and warmth likewiſe cannot be called Fire, ſince it can be produced in various manners without heat: under the ſame predicament are liver of ſulphur, ſome oils, varniſh of linſeed oil, iron-filings, &c. For theſe cauſe a heat, no doubt by means of the Air, by which ſome part of the Air is loſt (No. 51.); but the abſence of light prevents their being called Fire.

Remark 3. The luminous appearance of ſome kinds of ſtone, after they have been heated, the Bononian and Baldwin's phoſphorus, the electrical luminous appearance, and the light of the ſun, are likewiſe not to be conſidered as Fire; ſince the air is not changed by theſe phænomena, and they may equally well be ſeen in the vacuum of an exhauſted receiver. But the phoſphorus of urine is an actual Fire; for it is luminous, it heats, it is deſtroyed, and abſorbs Air. None of theſe phænomena appear in foul Air, or without Air in the exhauſted vacuum.—It is an improper expreſſion to ſay, water conſiſts of particles of ice and Fire; the Fire latent in ſome ſubſtances, the Fire of the ſun, &c.

76.

At preſent I intend to deſcribe my theory on the generation of Fire, and the phænomena it cauſes, founded on the preceding Experiments, and leave the deciſionther eon to the judgment of my readers.

1. *To every combuſtible body a certain quantity of heat muſt be communicated, in order to ſet it in the fiery commotion.* (a.)

(a.) Since heat is a very ſubtle, elaſtic, and fluid ſubſtance, it penetrates the pores of combuſtible bodies, and deſtroys their coheſion; the oils are changed into ſmoke, and the Air then finds an opportunity of coming in contact with them in more points, and conſequently a beginning is made of its deſtruction.—The weaker the coheſion is of the integrant parts of combuſtible bodies, the leſs heat is required in procuring ignition: phoſphorus requires only little

heat.—I cut about a drachm of phoſphorus in ſmall pieces, in order to examine whether the luminous appearance of this ſubſtance really produces a greater degree of heat than that of the common atmoſphere. I then put the ball of the thermometer in the middle between the ſmall pieces of phoſphorus. The ſpirit in the thermometer began to riſe, and after the ſpace of a quarter of an hour the phoſphorus conflagrated. One piece of phoſphorus does not take fire from itſelf; the increaſed number of ſurfaces communicating to the empyreal Air a greater bulk of phlogiſton, muſt naturally cauſe a greater heat; which explains the ignition.—Volatile vitriolic ether takes fire by holding a red-hot iron above it: In the ſame predicament is likewiſe the inflammable Air cauſed by iron or zinc and the vitriolic acid. Sulphur requires leſs heat than the fat oils. The water contained in common atmoſpheric Air is the chief cauſe of the ignition of pyrophorus, whereof I ſhall ſpeak afterwards.

2. *Then it is enabled to part with its phlogiſton, provided there be a ſubſtance preſent which has a ſtranger attraction to the inflammable than that with which it was before in union.* (b.)

(b.) If the phlogiſton be united with aërial acid, in that caſe the phoſphoric, nitrous, and arſenical acids, metallic earths, &c. are ſubſtances which deprive the aërial acid of its phlogiſton; however in this caſe, neither heat nor light is generated.

3. *If the heating is done in open air, the empyreal Air has the ſtronger attraction.* (c).

(c.) That empyreal Air has a very great affinity with the univerſal phlogiſton, has been demonſtrated in many paſſages of this memoir.

4. *Immediately after, the inflammable principle muſt come out and unite with the empyreal Air, and thus be ſet at liberty.* (d.)

(d.) In caſe the ſubſtance be coals or oil, it is the aërial acid; if ſulphur, vitriolic acid; if phoſphorus, phoſphoric acid; if

metals, metallic earths; which all part with phlogiſton, though not ſo as to acquire the higheſt ſtate of dephlogiſtication. Thus the vitriolic acid retains ſo much of phlogiſton as is neceſſary to generate ſpirit of ſulphur: Arſenical acid retains after the burning of the regulus, as much phlogiſton as makes common arſenic; (which makes it no wonder, that arſenical acid decompounds heat, and becomes arſenic.) There is no doubt of it that vitriolic acid is reduced by heat into volatile ſpirit of ſulphur: and the metallic calxes no doubt retain ſomewhat of phlogiſton.

5. *From this union heat is compounded, which adheres to foul Air, expands it, and riſes according to hydroſtatical principles.* (e.)

(e.) Heat is united to foul Air (No. 56. letter *m.*) For all the empyreal Air which had been mixed with it is united with the phlogiſton: if you collect Air paſſing thro' burning coals, a burning candle will immediately extinguiſh in it. Heat is not always generated by the compoſition of theſe two conſtituent parts, but it is found previouſly exiſting in moſt if not all bodies:

But who will believe, that in oily mixtures ſhould be contained all the heat that is felt, after they are brought into the fiery commotion? Only that kind of heat then is previouſly exiſting in the bodies, which without the addition of Air, in any way, may be extricated from them. It exiſts two ways in bodies; in the one, it fills the minute pores of bodies in which it enters juſt as a liquor reſides in capillary tubes. Secondly, it is united with certain ſubſtances, and makes one of their integrant parts; whereof I ſhall ſpeak afterwards.—The heat which is *contained in the pores* of bodies is there entirely inactive, ſince the attractive power of matter hinders its elaſticity; and all Experiments ſeem to indicate, that all the effects of heat on bodies muſt be aſcribed to their expanſion. This ſhut-up heat may be extricated in two different ways; either the minute pores muſt be ſtill more compreſſed, which is done by reciprocal friction of two bodies and the bending and hammering of metals; (for muſt not heat then come out, when the pores are opened on one ſide and compreſſed on the other, by the bending backwards and forwards of metals?) or the integrant parts of bodies muſt be disjoined, which is done partly by fermentation, putrefaction, and chemical ſolutions.

6. *Scarcely is this heat generated when the combuſtible body is ſtill more expanded by it than in the beginning, and its phlogiſton is more laid open.* (f.)

(f.) The more the heat is increaſed, the more minute are the particles into which the body is diſſolved; the empyreal Air meets with more ſurfaces, and conſequently comes in contact with more phlogiſton.

(7.) *Empyreal Air comes in contact with more phlogiſton, and, according to its nature, forms an union with a greater quantity of it, which cauſes radiant heat.* (g.)

(g.) Do we not obſerve, that vitriolic acid united with a ſmall portion of phlogiſton forms ſpirit of ſulphur; and, with a ſtill greater quantity, ſulphur. Arſenical acid has the ſame property; nitrous acid and metallic earth ſhew the ſame; and manganeſe, with a ſmall quantity of phlogiſton, becomes an abſorbent earth; with a greater portion, it forms a regulus. Empyreal Air is ſubject to the ſame laws.

8. *And at that very moment the integrant parts of the combuſtible body are ſo much diſunited by the ſtill increaſing heat, that the empyreal Air continuing to pour upon it in ſtreams, attracts the phlogiſton in ſtill greater quantities; and hence (that wonderful phænomenon) the moſt elaſtic ſubſtance,* Light, *is compoſed; which, according to the quantity of combuſtible matter, has various colours.* (h.)

(h.) Laſtly, when the great quantity of generated heat has ſo far diſunited the ſmalleſt particles of oily bodies that they can take up no more heat, it may be eaſily imagined, that even the integrant parts are disjoined; which may be done ſo much the more eaſy, the nearer a ſubſtance is found which is capable of attracting the phlogiſton in quantities; *viz.* the empyreal Air, which pouring in upon the combuſtible body in ſtreams, takes conſtantly up ſo much of the phlogiſton as is neceſſary to generate light. But ſince the phlogiſton cannot in all points thoroughly come in contact with the empyreal Air, on account of the acid quited by the phlogiſton, the empyreal Air muſt naturally take up different proportions of phlogiſton; and ſhould the

difference conſiſt even in a few atoms more or leſs, the properties ariſing from theſe different mixtures muſt be different, and ſhew us different colours, eſpecially when they are divided by the priſm. All theſe phænomena, *viz.* heat, radiant heat, and light, are produced in ſo quick a ſucceſſion that hardly a moment is paſſed after they are generated when in another inſtant they again as it were diſappear, and new ſucceſſions of heat and light are generated. The more Air is compreſſed the denſer is empyreal Air; the more it comes in contact with the combuſtible body, the more heat and light is generated, and conſequently the quicker is the combuſtible body reduced to aſhes; a ſtrong current and a pair of bellows ſhew this evidently.—Is there not a great quantity of phlogiſton in an oily mixture, ſo that empyreal Air may be, as it were, ſaturated with it? the light commonly then being tinged blue; as may be ſeen in the flame of charcoal, inflammable Air, ſulphur, and ſpirit of wine.—Certain ſtrange vapours in the flame ſeem to attract certain kinds of light: Should the vapour of copper not attract all the other beams of light, except the green ones; and the mineral fixed alkali all others, except the yellow, &c.?

Mr. *Meyer*, and ſeveral more, are of opinion, that light is already pre-exiſting in the combuſtible bodies, and only comes forth at their deſtruction. But the Experiments I have made upon light militate againſt this doctrine; and the following are likewiſe to the ſame purpoſe: When I, for inſtance, obſerve that *hepar ſulphuris* is deſtroyed in open Air, without the addition of heat, and I ſee no light preſent, (though light, even in its moſt ſubtle expanſion, may be well ſeen in the dark) I take an opportunity of inferring, that light, in the burning of ſulphur is only accidental.

I am ſtill more confirmed in this opinion, when I find that ſulphur is entirely diſſolved with efferveſcence, by fuming ſpirit of nitre poured on it, and kept in digeſtion; and yet no light appears.—If the ſolution be evaporated, nothing remains but a concentrated oil of vitriol.—Nay, even phoſphorus, treated in the ſame manner, with fuming ſpirit of nitre, is eaſily diſſolved, even without the addition of heat;

and no light appears in the operation. After evaporation there remains likewiſe only the pure acid of phoſphorus.

77.

In regard to the ſhining quality of ſome kinds of ſtone after they have been rubbed or heated, it ſeems to me probable, that their light is firſt compounded. There is not the leaſt doubt that the calcareous ſparry fluor, and many other kinds, contain ſome phlogiſton;—when theſe ſtones are heated, either by rubbing or heat, the phlogiſton unites with the heat, and by it the empyreal Air acquires more phlogiſton; and hence the light is generated: For it is indifferent whether empyreal Air attracts at once ſo much phlogiſton as is neceſſary for generating light, or whether heat attracts ſome additional phlogiſton, in order to compound this ſubtle elaſtic ſubſtance. Hence it likewiſe becomes evident, why this light may be generated in the *vacuum*, where Air has been extracted by the Air-pump; and why the ſparry fluor even ſhines in hot water. Were this light previouſly contained in theſe ſtones it muſt become viſible at their diſſolution. After the phlogiſton is extracted by heat, the ſhining is at an end; for which reaſon it is impoſſible ever to procure again light by the application of heat, after the ſparry fluor has once been heated red-hot and again grown cold.

Since the diamond entirely flies off in cloſe veſſels under a continued heat, ſhould not heat likewiſe unite with the great quantity of phlogiſton which the diamond muſt be poſſeſſed of, and carry it off under the form of light? The bright light which is obſerved during the calcination of diamonds, ſeems to give weight to this opinion.

In regard to the *Phoſphorus Balduini*, and the Bononian Phoſphorus, it is moſt probable, that theſe two ſubſtances attract the light from the ſun or fire. I cannot find the cauſe in anything elſe than in a certain magnitude of the moſt ſubtle pores, into which the particles of light penetrate, without being very ſtrongly attracted by the matter of theſe bodies; to which the acid of nitre or ſulphur exiſting in theſe bodies may contribute ſomething. Heat, which neceſſarily ought to be ſomewhat coarſer than light, when made very elaſtic by the addition of more phlogiſton, penetrates likewiſe into them; becauſe it is attracted more powerfully on account of its greater denſity, and therefore expels again the light: The more heat penetrates at once into the ſubtle pores, the quicker is the light expelled; and the brighter ſhines the phoſphorus.—Hence I collect the cauſe, why theſe *phoſphori*, after they are ſomewhat heated, do not attract the light, as long as they ſtill remain hot; becauſe then the particular pores are filled up by heat. Humidity has the ſame effect.

78.

A ſtone when put into fire firſt grows hot, and then red-hot; it therefore attracts from the fire not only heat but likewiſe light: the light, which at firſt penetrated into the ſtone or iron, together with the heat, is, by the attraction of the matter of iron, changed into heat, till all the pores are filled with heat; then the pores are expanded, ſubtle openings are made into which the light enters, and which is as it were preſſed into it by a new ſucceſſion behind; and this cannot any more be changed into heat, ſince the matter of the ſtone has no more attraction to ſpare: light therefore ſits very looſely; and may, whenever the ſtone is taken out of the fire, very eaſily ſtream out again; and this really happens; however, if in any manner after the ſtone is taken out of the fire, you ſuddenly take away its heat, the light ſooner diſappears: Plunge a red-hot iron, for inſtance, into water, the latter attracts the heat very quickly. I ſuppoſe this piece of iron would have been red-hot a quarter of an hour in the open Air; in water it will not be a minute red-hot, though the light is by no means attracted more powerfully by the water than by the Air. The cauſe of it is; as ſoon as the water has attracted the heat from the external ſurface of the red-hot iron, this ſurface attracts the light, and changes it into heat; ſo it happened when the ſtone or iron was firſt put into the fire.

Though

79.

Though nothing is more univerſally known, than that ſteel, ſtruck by a flint, yields ſparks, nevertheleſs, the true cauſe of this ignition is unknown. I ſhall, in the ſequel, prove by Experiments, that Fire contains a great quantity of heat, which has penetrated its pores. If then, by means of a ſharp and hard ſtone, a minute particle is *rapidly* ſeparated, the heat confined by this particle is ſet free, and adheres partly to it; the phlogiſton (which is known to be copious in iron) is by it enabled to unite with a ſubſtance which attracts it ſtronger than the earth of iron does, namely, the *empyreal Air* which is ready at hand; and by it the heat is ſo much increaſed, that ſtill more phlogiſton is ſet free, and *light* is then generated; in a word, the particle of ſteel conflagrates. All theſe phænomena follow one another in an inſtant.—If ſuch a ſpark falls on a looſe, ſpungy, and combuſtible body, it heats the place on which it alights, whereby its phlogiſton is let looſe, is attracted by the empyreal Air, and ignited.

If the red-hot particle of ſteel be ſomewhat larger, the included heat of its middle is then expanded by the external heat, and ſince the reſiſtance of a ſmall particle of ſteel cannot be otherwiſe than ſmall, it is disjointed by it, and divided into ſtill ſmaller ſparks: Theſe are thoſe ſparks which are ſo often ſeen to fly off laterally.—I ſaid, the particle of ſteel ought to be ſtruck off *rapidly*; for it may be eaſily imagined, that in caſe this happens to be done more ſlowly, the heat getting out of the pores is immediately again attracted by the ſtone, and by the whole piece of ſteel, as bodies infinitely more denſe than Air; and, conſequently, the phlogiſton cannot ſufficiently be ſet at liberty in order to unite with the empyreal Air.

80.

I had long deſired to be poſſeſſed of a portion of *Mercurius præcipitatus per ſe*, in order to try whether it would likewiſe yield empyreal Air during its reduction. At laſt I was favoured with ſome by my dear friend *Henry Gahn*, doctor of phyſic.

This ſo-called precipitate, had the appearance of ſmall dark red cryſtals, ſimilar to thoſe of cinnabar or vermillion. As I know very well, that mercury cannot be diſſolved in the marine acid, unleſs it be deprived of its phlogiſton, which may be done by its ſolution in nitrous or vitriolic acid; and is the cauſe why, in a mixture of calcined vitriol, common ſalt and quickſilver nitre ought to exiſt. I therefore poured marine acid on a portion of this red precipitate; the ſolution was ſoon done, and grew ſomewhat hot: I then evaporated it, and increaſed the heat; the whole was ſublimated, and formed a very corroſive ſublimate; conſequently this precipitate, prepared by mere heat only, is a calcined mercury. Another part of this precipitate I put into a ſmall glaſs retort, and having fixed to its mouth an empty bladder, I brought it into the fire; as ſoon as the retort became red-hot, the bladder was expanded, and the reduced mercury mounted into the neck. No red ſublimate was here mounting, as is the caſe with that calx which is prepared with acid of nitre. The Air obtained was pure *empyreal Air*.—This is a remarkable circumſtance, that empyreal Air, which before, in a ſlow calcination, had deprived mercury of its phlogiſton, likewiſe reſtores the ſame phlogiſton as ſoon as the calx becomes red-hot. But we know of more ſuch phænomena, where heat as it were changes the attracting powers of bodies.

81.

On Pyrophorus.

The accounting for the conflagration of this ſtrange chemical product, has by many been attempted in vain: They all agree in this, that therein exiſts a ſubſtance growing ſo hot in the open Air, that the coals contained in the pyrophorus are ignited. It has been thought, that a concentrated vitriolic acid is the cauſe of this heat; ſince moiſture accelerates the conflagration, and without this acid no pyrophorus can be made.But can it be demonſtrated, that a pure vitriolic acid, without any phlogiſton, is contained in the pyrophorus? and what is the reaſon, that oil of vitriol and water uniting, cauſe a heat? and why enſues no heat, when the pyrophorus is put into a foul and moiſt Air; though vitriolic acid is heated in the ſame Air by the addition of water?—We ſhall now examine, whether my Experiments with it are capable of explaining this curious and remarkable phænomenon.—I had treated clay with oil of vitriol, in order to make alum, and I really produced alum without the addition of alkali; there remained however, a thick *magma* (maſs) which would not cryſtallize. Part of this *reſiduum* I employed in making a pyrophorus: Having calcined this in the uſual manner, I found to my aſtoniſhment, that it would neither kindle in the open Air, nor even cauſe the leaſt heat. Hereupon I took the other part, put ſome alkali of tartar to it, and calcined it in the uſual method, and I obtained a fine pyrophorus.—I learned by it firſt and foremoſt, that a fixed alkali is neceſſary to its generation, which unites with the ſulphur generated in the operation; and, conſequently, the liver of ſulphur is the chief ſubſtance in this product. I knew very well that hepar ſulphuris grows not hot in the common Air; but I ima-

I imagined, however, that being mixed with the porous earth of alum in the pyrophorus, it might grow ſenſibly hot. In conſequence, I mixed a ſtrong ſolution of liver of ſulphur with burnt alum, and calcined it violently in a cloſe glaſs; but after it had been cooled, I found that it would not heat in the open Air.—I repeated the Experiment; only with this difference, that I added ſome charcoal-duſt to the mixture, and behold, after having gone through the calcination, I had a good pyrophorus. I collected from thence, that a liver of ſulphur was not the only thing neceſſary, but that *coals* alſo were to be in the mixture. I next mixed a ſpoonful of vitriolated tartar reduced to powder, with three ſpoonfuls of charcoal, reduced to a powder, and calcined the mixture in the uſual manner with a ſtrong fire: After it was grown cold, I found a fine pyrophorus. Hence it follows, that ſince without a fixed alkali no pyrophorus can be generated; and ſince alum joined with the volatile alkali, cryſtallizes, this muſt be certainly the cauſe why every alum yields not a pyrophorus.—It was ſtill neceſſary to aſcertain whether *moiſture* was abſolutely neceſſary, in the kindling of the pyrophorus. I made a very dry Air, by putting ſome very ſmall pieces of quick-lime into a ſmall matras; then I put the neck of another matras into that of the firſt, ſo that the Air of both might communicate, and I luted the crevices with wax. Two days after, I ſeparated the empty matras, and poured half an ounce of pyrophorus from my phial into it, and immediately carefully ſhut the aperture up; but I did not obſerve that it grew in the leaſt warm. An hour after this, I put a ſponge moiſtened in water into the matras, and ſhut it again up: A few minutes after the pyrophorus began ſtrongly to be heated, and ſome pieces kindled ſpontaneouſly. — Then I filled a matras with *foul Air*, and put ſome pyrophorus into it, and put likewiſe a moiſtened ſponge into it, but the heat did not appear: When I poured it afterwards out, it kindled immediately.

How is it then with this kindling? Pyrophorus is generated from hepar ſulphuris and charcoal: The liver of ſulphur attracts the phlogiſton ſet looſe from the coals by making it red-hot. (I ſhall prove in the ſequel, that ſulphur is capable of taking up ſtill more phlogiſton). This ſubſtance compounded of alkali, phlogiſton, and ſulphur, will not kindle without moiſture and empyreal Air: alkali which attracts moiſture ſtrongly, is diſabled by it from keeping any longer the phlogiſton; eſpecially if there be preſent a ſubſtance attracting it more powerfully, viz. empyreal Air, which preſents itſelf and unites with the looſely adhering phlogiſton; this forms heat; which, by the help of new ſupplies of empyreal Air, is quite ſufficient for kindling both the ſulphur and the coals; and ſince after the burning of the pyrophorus no liver of ſulphur is to be found, it muſt have been calcined during the heat. If the pyrophorus be thrown into water, before its kindling, an hepatic ſolution is formed, precipitating the *Acetum lithargyrii* black; whereas a ſolution of common liver of ſulphur yields brown precipitate. I obſerved likewiſe, that the firſt ſolution abſorbed Air quicker than the laſt; it muſt therefore contain a great quantity of phlogiſton.

The conflagration produced by a mixture of pounded ſulphur and iron-filings moiſtened together, muſt, as I ſuppoſe, be explained in the ſame manner. Experiments taught me, that iron and ſulphur will not intimately unite, unleſs ſome part of the phlogiſton be ſeparated from the metal. Hence it is evident, that the effort of the earth of iron to unite with ſulphur muſt be ſtronger than that to unite with the phlogiſton; if then a ſubſtance be preſent which can unite with the ſeparated phlogiſton, there muſt ariſe effects proportionate to the union of the two ſubſtances.

Take three parts of freſh iron-filings, one part of the fineſt powder of ſulphur, and ſo much water as is neceſſary to make a ſtrong dough of this mixture. The water begins to act firſt upon the iron, whereby its phlogiſton is freed of its fetters; the ſulphur increaſes this action, and unites with the half-dephlogiſticated iron, (which gives to the whole mixture a black colour;) the diſpoſſeſſed phlogiſton ſits on the ſurface ſo looſely adhering, that it may be ſoon expelled, (No. 54). If the Air is preſent, the empyreal Air contained in it attracts it; and heat is then compounded, which is proportionate to the extent of ſurface and the little coheſion of the mixture; which from the conſtant ſupply of empyreal Air, muſt ſo much increaſe, that the quantity of ſulphur kindles, and the whole maſs becomes calcined.—But what becomes of the phlogiſton of the iron, in caſe it unites while on fire, with ſulphur in cloſe veſſels? for if this melted maſs be reduced into a minute powder, and be moiſtened with ſome water, nevertheleſs it will not become heated in the Air? If the phænomena are attended to, produced by this mixture during its union on the fire, it will be eaſy to anſwer this query. It has been obſerved, that almoſt every metal capable of undergoing with ſulphur an union on the fire, kindles in the very moment when this happens: and tho' the preſent operation be done in cloſe veſſels, ſome phænomenon ſimilar to this happens. I mixed three ounces of clean iron-filings with one ounce and a half of ſulphur in powder, and put it into a glaſs retort, three fourths of which were filled by it; and fixed to its neck a bladder emptied of Air and moiſtened, (No. 30. letter *b.*) and then I gradually put the retort on burning coals. When the retort began to be red-hot below, the maſs itſelf began to be red-hot about the margin; this fine purple red light extended more and more, till even the very middle of it was red-hot; then the margin became again dark, and at laſt the purple light diſappeared even in the middle. All this happened, though the retort continued to remain in the ſame fire. During this phænomenon, as much Air went over and expanded the bladder as would fill eight ounces meaſure. It was an inflammable Air, and had no ſmell.

I have already demonſtrated, that light is diſtinguiſhed from heat by a greater quantity of phlogiſton only. Here in the retort is nothing wherewith the phlogiſton of the iron, expelled by the ſulphur, could be united; by the increaſing heat of the Fire it is entirely deprived of that ſmall degree of coheſion, whereby it might even adhere to the outer ſurface of the mixture; and ſince it cannot be ſeparated from one ſubſtance unleſs there be another with which it can immediately unite (No. 27—5.) the heat penetrating through the retort attracts the phlogiſton, and what could be produced but light? As much light is generated, as the phlogiſton expelled frôm the iron will permit; and when this is conſumed, the luminous appearance muſt be at an end.—But whence comes the inflammable Air in the bladder? I have already mentioned before, and ſhall demonſtrate it hereafter, that this inflammable Air is compoſed of heat and a greater portion of phlogiſton than is neceſſary to generate light. This ſuppoſed, it therefore follows, that in the union of iron and ſulphur, light is generated; but ſince there is more phlogiſton extricated than is neceſſary for that purpoſe, the ſuperfluous part of it, united with a ſmall portion of heat, generates inflammable Air.—I ſhall introduce ſome Experiments which may convince thoſe who have doubts againſt this theory. I mixed *crocus martis* with half of its weight of ſulphur and diſtilled as before; in this operation I obſerved no ignition, and no Air was in the bladder; but I found a volatile ſpirit of ſulphur. The *crocus* was become black and was attracted by the magnet; and it contained very little of ſulphur, ſince this was altogether gone up into the neck. Hence it becomes evident, that the earth of iron entirely deprived of its phlogiſton, attracts it only to a certain point ſtronger than the vitriolic acid; which produces the volatile ſpirit of ſulphur. It may, however, be ſeen from this Experiment, that the ſmall quantity of phlogiſton is not ſufficient for uniting the earth of iron with ſulphur; it ſhould contain ſomewhat more of it; whereas the metallic iron poſſeſſed too much of it. I mixed likewiſe this earth of iron with ſulphur and water, ſo as to form a dough of it. But this mixture became neither black nor warm in the open air.—I diſtilled a mixture of lead-filings and ſulphur. Here likewiſe ſuch a dark red light appeared; however, ſince the lead contains leſs phlogiſton than the iron, it is no wonder that I could collect no Air in the bladder. Here it likewiſe appears, that *part* of the phlogiſton is expelled from the lead by the ſulphur; which produces, with heat, light. I ſay on purpoſe, part of it; for if a calx of lead be diſtilled with ſulphur, a volatile ſpirit of ſulphur is formed, and *potters-ore*; (*Plumbum galena. Linn.)* conſequently the calx of lead muſt previouſly be united with ſome phlogiſton before it can undergo ſuch an union with ſulphur.

82.

On Fulminating Gold.

At preſent I come to another ſtill more remarkable phænomenon, which the fulminating gold preſents us with. Am I ſo happy as to have found the true cauſe of this phænomenon? No; I will not flatter myſelf with this hope, but rather take notice of what my readers may ſay of the concluſions deduced from my Experiments.—That gold is a compound of an earth ſui generis and of phlogiſton, probably nobody will call in queſtion; and that gold is not ſoluble in acids, unleſs it has parted with its phlogiſton, is likewiſe known and confirmed by the moſt deciſive Experiments. The marine acid has, of all acids, the greateſt affinity to the earth of gold: however, it cannot unite with the earth of gold, unleſs there be preſent another ſubſtance capable of depriving gold of its phlogiſton: This is effected by the ſpirit of nitre; whoſe acquired volatility by this means viſibly demonſtrates it. The gold is then attacked by two powers at once, and its ſolution is brought about.— But gold may likewiſe be diſſolved in dephlogiſticated marine acid, which has as powerful an attraction to the phlogiſton as the acid of nitre: In what manner ſuch acid may be obtained I have ſhewn in my memoir on mangaueſe (No. 64). Such a ſolution of gold contains pure marine acid; becauſe it has recovered from the gold the phlogiſton which it had before depoſited upon the manganeſe; nevertheleſs, if the gold ſolution be ſtrongly diſtilled in a retort, the gold is reduced, and the marine acid goes over in its original ſtate, (viz. dephlogiſticated) into the receiver: the cauſe is, that the earth of gold, by means of the heat, acquires a ſtronger attraction to the phlogiſton, and deprives the marine acid again of it.—In the ſame memoir I have likewiſe demonſtrated, that from this ſolution, by the volatile alkali fulminating gold is generated. This phænomenon removes already a great difficulty; ſince by this it appears that acid of nitre is by no means neceſſary in the generation of fulminating gold. When I find that the earth of gold is ſeparated from its ſolution in its metallic ſplendor, I am ſure that it has acquired phlogiſton ; metals precipitate gold in its ſplendent form, but not their calxes: Fixed alkalies decompound the ſolution of gold, but it ſucceeds very ſlowly, and the precipitate thus obtained, I call the earth of gold : the volatile alkali precipitates the ſolution of gold quicker, and this precipitate is the object of this ſection.

Earth of gold can unite with a part of volatile alkali, and then forms a kind of analogous ſalt.

I digeſted thirty grains of earth of gold, with ſome ſpirit of ſal ammoniac prepared with lime ; I then edulcorated this earth, and dried it gently ; it weighed thirty-ſeven grains, and was changed into fulminating gold. From a theſis defended at *Upſal*, under the *præſidium* of Profeſſor *Bergmann*, I find that the ammoniacal ſalts likewiſe communicate to the earth of gold the property of fulminating ; I have been guided by this excellent *theſis* ; and my enquiries have in many points been facilitated by it.—I put a ſolution of Glauber's ſal ammoniac * with the earth of gold into digeſtion; then I obſerved that this ſolution was become acidulous; which proved that the volatile alkali had been united with the earth of gold. After elutriation I found this earth to be a real fulminating gold: hence it is evident, that the volatile alkali has a nearer affinity to the earth of gold than to the acids.

I diſſolved a well waſhed fulminating gold in marine acid; into this ſolution I put ſeveral pieces of copper: the gold fell in the form of a minute powder to the bottom of the phial. I then filtered the ſolution; evaporated it to dryneſs; then mixed with it ſome ſalt of tartar; and after diſtillation I found a real volatile alkali in the receiver.—That the earth of gold unites with alkaline ſalts is nothing extraordinary, ſince many more metallic earths are poſſeſſed of the ſame property; and this confirms, in ſome meaſure, the opinion mentioned before, (No. 73) that all earths are ſpecies of acids.

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* Glauber's ſal ammoniac is formed from acid of vitriol and volatile alkali.

A

A kind of Air is generated in the conflagration of fulminating Gold.

I took a glaſs tube of the thickneſs of a finger and the length of a foot, and ſomewhat tapering at one end. I put the tapering part ſo far into water, that about one third of it remained empty; I then ſtopped up the part under water, took it out of the water and marked the height of the water in the tube: then I kept the tube in an oblique direction, inclined towards the horizon, and put in at the upper end about one grain of fulminating gold; taking care however to preſerve this empty part dry from the water; and laſtly ſtopped this end likewiſe up with a well-fitting cork; after this I held the tube in the ſame direction over a burning candle, and heated the place where the fulminating gold was laying. After the ignition of it, and ſome hours after the tube being quite cold, I opened the tapering end, when ſome water ran out. I repeated the Experiment ſeveral times with the ſame ſucceſs.—The Air generated took up as much room as one drachm and a half meaſure of water; then I was deſirous of learning what *kind* of Air the newly produced one might be. I mixed carefully half a drachm of fulminating gold with three drachms of vitriolated tartar; this powder I put into a ſmall glaſs retort, and fixed to its neck a bladder emptied of Air: then the retort was put on burning coals. After the mixture had been heated, it became dark brown; ſome moiſture got up into the neck, and ſome little of a white ſublimate, and the bladder was expanded. When the retort was grown cold, I tied up the orifice of the bladder, and removed it from the retort: the raiſed ſublimate weighed about two grains, and was a common ſal ammoniac; the Air in the retort was impregnated with a ſmell of volatile alkali. On the *reſiduum* I poured hot water, which diſſolved the vitriolated tartar; and there remained a brown powder, which was a ſubtle gold, which had undergone reduction in the form of a powder; the air in the bladder ſmelt ſtrongly of volatile alkali, took up the room of ſix ounce meaſure, and had the following properties: 1. It was not miſcible with water; 2. It did not precipitate lime-water; 3. It extinguiſhed immediately the flame of a burning candle; and was conſequently Air, in every reſpect, ſimilar to that generated by the deſtruction of volatile alkali. I have recited remarkable demonſtrations of ſuch kinds of deſtructions of the volatile alkali in my *memoir on manganeſe*: The principal points of it ſhew, that whenever a body attracts the phlogiſton which is an integrant part of the volatile alkali, ſuch Air is always produced. I afterwards have procured ſuch Air by various methods, viz. from a mixture of crocus martis and ſal ammoniac, which were diſtilled in a retort with a bladder fixed to it; and from the white precipitate of ſublimate mercury, made with volatile alkali; this precipitate conſiſting of the earth of mercury, ſal ammoniac, and ſome water. The Air procured by the detonation of nitrum flammans is for the greater part the ſame.

In order to aſcertain whether our Air contributed any thing to the accenſion of fulminating gold, I filled a glaſs with aërial acid, and put a little of fulminating gold into it, ſtopped it up, and put it in the dark on hot ſand; but it kindled it the ordinary way.

From theſe Experiments I form this concluſion; ſince fulminating gold is a compound of volatile alkali and gold earth, and to the accenſion of fulminating gold heat is always required, and heat is a compound of phlogiſton and empyreal Air, and gold earth attracts ſtronger the phlogiſton than empyreal Air, (No. 39) heat muſt become the cauſe of the reduction of fulminating gold: however, ſince in that caſe the empyreal Air is ſet free, it immediately unites with the combuſtible of the dry volatile alkali, becauſe this ſalt has no affinity with gold*; and ſince there is more phlogiſton preſent than is neceſſary for producing heat, the always-ſhining light is generated: the Air now ſet free from its union with the phlogiſton in the alkali volatile, acquires again its elaſticity; which is ſtill more increaſed from the extracted moiſture, ſal-ammoniac and ſome volatile alkali, which are

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[*] Volatile alkali has one of the leaſt affinities with gold in its metallic form, except water. See Chevalier Torbern Berymann's Table of Affinities, in the 21ſt column. In the ſecond Volume of the Nova Acta Upſalienſia.

panded into elaſtic vapours by the ſuddenly procreated heat; it therefore gives an impulſe to the Air all around, which by it is put into ſuch an undulated motion as is required to produce ſound.

In regard to the ſal-ammoniac produced during diſtillation, I am of opinion that it does not belong to the fulminating gold; for undoubtedly there muſt have been ſome ſmall portion of marine acid, with the fulminating gold; which, by the addition of the ſeparating volatile alkali, formed a ſal-ammoniac.—I likewiſe am of opinion, that with the fulminating gold a greater portion of volatile alkali exiſts than can deſtroyed by the empyreal Air. That volatile alkali can be deſtroyed by empyreal Air, may be collected from hence, that whenever a piece of it is thrown into a red-hot crucible, it is immediately kindled and flames.—I am alſo of opinion, that were it only poſſible to unite the earth of gold intimately with charcoal, it would form a fulminating gold. I put a portion of earth of gold mixed with ſome duſt of charcoal into a ſmall glaſs, and ſet it on hot ſand; inſtantly the gold earth was reduced, and the coals were kindled: That the heat of the ſand could not be the cauſe of the accenſion of the coals I found out by ſtrewing part of this duſt of charcoal on the hot ſand, which was not kindled; but it would, however, moſt certainly have kindled, if the empyreal Air had been preſent in great quantity.

83.

Air is a Dulcified Elaſtic Acid.

In the preceding Experiments I have recited two of the conſtituent parts of common Air, becauſe it was not neceſſary for a diſtinct idea of fire to know more of it: Now I intend to advance one ſtep further, and examine *whether a more detailed analyſis of Air is not to be obtained.*

Experiment I.

I put a rat into a matras of four quarts meaſure; I gave it ſome bread ſoaked in milk, and ſhut up the matras with a moiſt bladder: one and thirty hours after it died;

Then I turned the matras, put the orifice under water and cut a hole into the bladder; when two ounces of water mounted into the matras. The ſmall diminution of Air is probably owing to the heat which was in the rat, and had previouſly expelled the Air.

84.

Experiment II.

I took a large ſoft bladder and fixed a tube to its mouth; then I filled it with the Air contained in my lungs, and ſhut up the tube and bladder with my right hand, and with the left I ſhut up both my noſtrils; I reſpired the Air in the bladder as long as ever I could, and could make twenty-four inhalations, (here it ought to be obſerved, that at laſt I was obliged to inhale at once all the Air of the bladder, whereas at the beginning I wanted no more than one half of it for one inhalation): I then ſtopped up the tube with my finger, and tied up the bladder. The Air had the ſame qualities as that wherein the rat had died, viz. it contained about one-thirtieth part of aërial acid, which was ſeparated by the milk of lime; and a burning candle was immediately extinguiſhed in it.

85.

Experiment III.

I ſhut up ſome flies in a phial, into which I had laid a paper whereon I put ſome honey: ſome days after they all were dead; but they had not abſorbed any Air: The milk of lime diminiſhed its bulk of about one-fourth part, and the reſt extinguiſhed a candle.

Then I took a bottle of twenty ounces meaſure, and near its bottom I bored a hole into it with the corner of a broken file (fig. 5, A.); into this bottle I put a ſmall piece of quick lime, and ſhut its mouth with a cork, through which I had paſſed a tube, B; round the cork I laid a ring of pitch, and having put a bee with ſome honey on a paper into a common open glaſs, C. I ſet it inverted on the pitch, (into which I gently preſſed the glaſs, in order to exclude all communication of external Air;) then the bottle was placed in a diſh, D; this I filled ſo far with water as to cover half of the bottle; finding the water lifted the bottle off from the bottom of the diſh, I laid a ſmall weight on the top of the inverted glaſs. The water mounted every day into the bottle by the hole, A. I took care now and then to ſhake the bottle, in order to burſt the ſkin formed on the ſurface of the water. At the end of ſeven days the water was riſen to E. and the bee was dead: Sometimes I put two bees at a time into the glaſs, C. when the ſame quantity of Air has been changed into aërial acid in half the time. Caterpillars and butterflies cauſe the ſame effect.

86.

Experiment IV.

In a ſmall matras of twenty-four ounces meaſure I put ſome peaſe, and poured ſo much water on as to cover them half;

then I ſhut the mouth of the matras. The peaſe began to ſtrike out roots and to grow: When I ſaw after a fortnight that they would increaſe no more, I inverted the matras, and opened its mouth under water; and found the Air neither increaſed nor diminiſhed. With the milk of lime one fourth of the Air was abſorbed, and the reſt of it extinguiſhed a burning candle. I have preſerved freſh roots, fruit, herbs, flowers, and leaves, each ſeparately in a matras; and found after ſome days one-fourth of the Air changed into aërial acid: flies put into ſuch Air died immediately.

87.

Theſe are remarkable circumſtances; that Air is not conſiderably abſorbed by animals endowed with lungs, contains very little aërial acid, and however extinguiſhes the flame of fire; whereas inſects and vegetables ſo greatly change the Air, that one-fourth of it becomes aërial acid. I was therefore deſirous of knowing whether *empyreal Air is not that which is here changed into* *aërial acid,* becauſe juſt the ſame bulk of Air had been converted into aërial acid which empyreal Air occupied in it.

88.

Experiment V.

In a bottle of twenty ounces meaſure I mixed one part of empyreal Air, with three parts of Air wherein peaſe had ceaſed to grow, and from which the aërial acid had been ſeparated. I filled the bottle with water, and put four peaſe into it; then I poured into the bladder containing the empyreal Air, the fourth part of the water; and into another bladder including the foul Air, I put the reſt of it, (No. 30, letter *g.*); I took care at the ſame time that the peaſe ſhould not fall into the bladder, and I likewiſe left ſo much water as would cover half the peaſe. I obſerved the peaſe growing; and when they would grow no more, I found the Air likewiſe not abſorbed; but nearly the fourth part of it was abſorbed by the milk of lime. It is therefore the empyreal Air, which is changed into aërial acid. In three parts aërial acid and one part empyreal, the peaſe will not grow.—I have mixed foul Air (No. 29) with empyreal Air, and found the ſame event; viz. the empyreal Air was changed into aërial acid.

89.

Experiment VI.

I mixed in the ſame proportion Air corrupted by peaſe, with empyreal Air; and filled a bladder with it. Having exhaled the Air contained in my lungs, I breathed this newly compounded Air as many times as was poſſible: I found that it contained very little aërial acid, and (like that) extinguiſhed the flame of fire. I am of opinion that the effect which animals endowed with lungs upon Air, ought to have be aſcribed to the *blood* contained in the pulmonary *veins*. The following Experiment occaſioned this opinion:

It is known that blood newly taken, and expoſed to common Air, acquires on its ſurface a fine florid red; and that the parts below, after coming in contact with Air grow likewiſe red: Does the Air perhaps here undergo an alteration?—I filled one third of a matras with blood recently taken from a bullock; and tied it carefully up with a bladder, and ſhook it frequently: Eight hours after this, I found in this Air neither aërial acid, nor any diminution of it in bulk; but the flame of a candle was immediately extinguiſhed in it. This Experiment was made during winter; Whence it is clear, that the above effects cannot be aſcribed to putrefaction; ſince the ſame blood ſix days after was ſtill found to be freſh, and all putrefactions produce aërial acid.—I was now deſirous of knowing how empyreal Air by itſelf, would act upon animals and vegetables.

90.

Experiment VII.

a. I put two ounces of ſaltpetre in a ſmall glaſs retort on burning coals, and fixed to the orifice a large ſoftened bladder; (No. 35.) then I left the ſaltpetre ſo long boiling, that I had obtained in the bladder three-fourths of a quart of empyreal Air: I tied up the bladder, removed it from the retort, and fixed a tube to its orifice; and having quite emptied my lungs from Air, I began to breath the Air out of the bladder, (No. 84). This ſucceeded ſo well, that I was able to take *forty* inhalalations before it became troubleſome to me; and laſtly, exhaled all the Air from my lungs into the bladder. The Air ſeemed not much to be diminiſhed, and having filled a glaſs with it, a burning candle introduced into it continued to burn.

b. I was aſtoniſhed that I could not at the very firſt trial deprive the Air of its property of bearing a burning candle in it: I imagined that perhaps the quantity of humidity might hinder me from inhaling this Air ſo often as was poſſible; I therefore repeated the Experiment, with this difference only, that I put about a handful of pot-aſhes into the bladder before I forced the empyreal Air into it; then I began to inhale this Air into my lungs, and counted *ſixty-five* inhalations before I was obliged to leave off; but immerſing a burning candle into this Air, it would burn, though only a few ſeconds.

91.

Experiment VIII.

I ſhut up the ſmall hole A. (fig. 5), with a cork, and likewiſe the tube B. and filled the glaſs with empyreal Air, (No. 30. letter *c.*) Then I was ready with the glaſs; into which I put two large bees and ſome honey for their ſuſtenance. I opened the tube hitherto ſhut, and as ſoon as poſſible I covered it with the glaſs, and preſſed it into the ring of pitch. Hereupon I ſet the caſe in the diſh D. which I had filled with milk of lime, I then drew out the cork at A. I obſerved the milk of lime mounting ſomething every day into the *Apparatus*; and when eight days were elapſed, the glaſs was very nearly filled with it; and the bees were dead.

92.

Experiment IX.

Vegetables will not thrive well in empyreal Air. I filled with this Air, a phial of ſixteen ounces meaſure, containing four peaſe; (88.) they got roots, but would not grow up. With milk of lime the twelfth part was abſorbed.—This Air I removed into another phial, containing likewiſe four peaſe. A fortnight after they had ſtruck out roots, but were not grown, and with the milk of lime, only one-twelfth part was abſorbed. I repeated the ſame Experiment ſtill three times more, with the ſame Air; and I obſerved that the fourth and fifth time the peaſe had begun to grow upwards: The half of the whole Air remained, and the flame of fire would ſtill burn in it. There is not the leaſt doubt, had I continued the operation, that at laſt the whole quantity of empyreal Air would have been converted into aërial acid. It ought likewiſe to be noticed, that when the peaſe began to ſtrike roots, they acted more powerfully upon the empyreal Air than afterwards.

93.

It is therefore empyreal Air, by the means of which the circulation of the blood, and of juices in animals and plants, is ſo much ſuported : But it is a peculiar circumſtance, that the blood and the lungs have not ſuch effects on empyreal Air as inſects and vegetables ; ſince the laſt change it into aërial acid, and the former into foul Air. (No. 29, 89, 90.)—It is not eaſy to account for this, though I will attempt to do it. It is notorious that all acids loſe the properties by which they prove themſelves to be acids, on the addition of an inflammable, (as may be evidently ſeen by ſulphur, elaſtic acid of nitre, regulus of arſenic, ſugar, &c.); now I am apt to ſuppoſe that empyreal Air is compounded of a ſubtle acid united with phlogiſton, and that it is probable that all acids owe their origin to empyreal Air. If this Air penetrates vegetables, they muſt attract the phlogiſton; and conſequently the acid which manifeſts itſelf as aërial acid, muſt make its appearance again and part from it. The objection, however, that at the deſtruction of vegetables a great quantity of aërial acid is obtained, in conſequence of which they ought to attract the aërial acid, has no weight; for in that caſe the Air in the veſſels which contained the peaſe, muſt have been loſt for the greateſt part; which however did not happen. It ought to be remembered what I have proved about the conſtituent parts of heat and light; and ſince without heat no vegetable can well thrive, it is eaſy to be conceived that heat and light is decompounded in vegetables; ſince there is nothing elſe required than that the inflammable principle may be ſeparated from theſe ſubtle ſubſtances by the fineſt capillary veſſels, and by the preſervation of a very little of acid, and the addition of a few atoms of water changed into oil. The generation of a green roſin in plants, after having been brought out of a cellar where they are almoſt white, and ſtanding a couple of days only expoſed to ſunſhine; the production of inflammable Air, which is nothing elſe but a very ſubtle oil, make this probable to me. If the plants attract the phlogiſton from the Air, the aërial acid muſt be lighter; but experience ſhews the contrary; I found it after a moſt ſcrupulous weighing ſomewhat heavier: But this by no means militates againſt my opinion, ſince it is well known that as all the acids ſtrongly keep the water, the aërial acid muſt likewiſe be poſſeſſed of the ſame property; and this might be the cauſe of the heavineſs.—If all this be conformable to truth, there ariſes a new query: Why the blood and the lungs do not change the empyreal Air into aërial acid? Here I likewiſe take the liberty of communicating my opinion about it: (For what utility could the Experiments have which I made with ſo much trouble, did they not give the moſt grounded hopes of obtaining at laſt my intention, viz. to come nearer to truth?) Phlogiſton, which communicates for the greater part to thoſe ſubſtances with which it is united, fluidity, mobility, and elaſticity; muſt have the ſame effect on blood. The globules of the blood muſt attract it from the Air, through the ſmall pores of the pulmonary veſicles; by this union they are ſeparated, and conſequently made more fluid, and appear then very red (No. 89.) They ought however to depoſit this attracted phlogiſton during circulation; and conſequently be enabled to imbibe again from the Air, the phlogiſton in the place where they come the neareſt in contact with it, *i. e.* in the lungs. What may have become of this phlogiſton I leave to others to determine. The attraction the blood has to the phlogiſton is not ſo powerful as that of plants and inſects attracting it from the Air, and therefore the blood cannot change ſuch Air in aërial acid; it is however changed into an Air which is a mean between empyreal Air and aërial acid, viz. foul Air; for it unites neither with lime nor water, as empyreal Air does, and extinguiſhes fire in the ſame manner as aërial acid. However to prove that blood does attract phlogiſton, I have another Experiment in ſtore, when I deprived inflammable Air (by means of my lungs) of its phlogiſton, and changed it into foul Air.

I filled a bladder with Air produced from iron filings, and acid of vitriol, (No. 30, letter *c.*) and inhaled it in the manner deſcribed before (No. 48.) I could inhale it no more than twenty times; and having again recovered in ſome meaſure, I again expelled the Air from the lungs as much as poſſible, and then drew in again this inflammable Air. After ten inhalations I found myſelf obliged to leave off, and found that it was no more inflammable, nor would it unite with lime-water; in a word, it was foul Air.

I kept a piece of ſulphur in a retort of twelve ounces meaſure for half an hour, conſtantly boiling over the fire; and in lieu of a receiver I fixed an empty bladder; and beſides this I ſo placed the retort that the ſulphur mounting in the neck might run back. After every thing was grown cold again, I found the Air neither increaſed nor decreaſed; it ſmelt ſomewhat ſimilar to liver of ſulphur, and extinguiſhed the flame of a candle. That phlogiſton can unite with a greater quantity of ſulphur I ſhall prove hereafter; and it ſeems to be concluſive from this Experiment, that ſome phlogiſton of the Air had been depoſited on the ſulphur, and had acquired the property of foul Air.—It is however likewiſe remarkable, that other ſubſtances more powerfully attracting the phlogiſton, as (for inſtance, fuming ſpirit of nitre) do not deprive the Air of it.—It is equally wonderful, that I could inhale inflammable Air only twenty times into the lungs; and I obſerve here, as ſomething peculiar, that if I am not miſtaken, I found myſelf about a quarter of an hour afterwards conſiderably warmer.—It muſt likewiſe be remarked, that empyreal Air made foul by the lungs extinguiſhes fire.—Why does aërial acid not attract the *phlogiſton* again? Why likewiſe not foul Air?—Mr. Prieſtley ſucceeded in this; but I could not bring it about, though I ſhould have liked it very much: He has changed aërial acid, with a mixture of iron filings, ſulphur, and ſome water, into ſalubrious Air; whenever I wanted to imitate this Experiment, I conſtantly found the aërial acid abſorbed in the iron filings. I have likewiſe taken iron filings melted together with a ſuperfluity of ſulphur reduced to a powder, and moiſtened with water; and kept it cloſely ſhut up in a bottle filled with aërial acid, but with the ſame ſucceſs: The aërial acid was in two days almoſt entirely abſorbed.—This learned man likewiſe ſays, that he made foul Air again good by ſhaking it with water: but I muſt confeſs that I conſtantly failed in this. I filled the fourth part of a matras with foul Air, and the reſt with freſh water, I carefully ſhut the matras and ſhook it up and down for almoſt an hour; and when I collected this Air into a bladder, and from thence into a glaſs, I found that a candle would no more burn in it than before.—He mixed the inflammable Air from metals, by ſhaking, in water; nor has this ſucceeded with me; though I uſed only a very little of Air, and a great quantity of water.—He ſays likewiſe vegetables make foul Air again ſalubrious: From my Experiments the reſults were that they corrupt Air. I have kept vegetables in a matras filled with foul Air and ſhut it well (where the well-ſhutting-up muſt be well obſerved,) both in the dark, and likewiſe expoſed to the light of the ſun; I tried ſome ſmall part of this Air every two days, and always found it foul.

94.

Water has the peculiar quality of diſuniting the next conſtituent parts of Air in order to unite with empyreal Air, and never to enter into union with foul Air.

1. I filled a large bottle with boiled water, which a little while ago had grown cold, and let one tenth part of it run out; then I ſet this bottle inverted and open in a veſſel with water: I found the bulk of water diminiſhing ſomething every day; and when it had done diminiſhing, I collected the remaining Air firſt in a bladder, (No. 30, letter *b.*) and from the bladder into a glaſs (No. 30, letter *e.*) and then introduced a burning candle into the glaſs, which was extinguiſhed the moment it reached the aperture.

2. Then again I took water whoſe Air had been expelled by boiling; filled with it a bottle, and let one tenth part of it run into a bladder filled with foul Air; then I ſet the bottle inverted into a veſſel with water, and obſerved the ſpace the water occupied in the bottle; a fortnight after I found that the water had not abſorbed any of it.

3. I put a large bottle of which the bottom was broke off into a deep kettle, with water which covered the mouth of the bottle; then I tied a bladder emptied of Air to the mouth of the bottle, and

made the water boil. The Air contained in that part of the water, which was contained under the bottle, roſe into the bladder; and having tied up the orifice of the bladder, and detached it from the mouth of the bladder, I filled a glaſs with it, and introduced a ſmall burning candle into it; which burned brighter than in common Air.

This empyreal Air diſſolved in water, muſt be equally indiſpenſible to aquatic animals as to thoſe who live on the earth: they muſt inhale it into their bodies, and change it either into aërial acid or foul Air. Of whatever kind it be, it muſt always again ſeparate from the water; for, as aërial acid, it does not remain with water in the open Air, and foul Air cannot at all be united with it (No. 2). The water is capable of diſſolving empyreal Air in the firſt inſtance and of communicating it to animals.

The Experiments I made in this reſpect correſpond with it.--I let ſome leeches ſtand in a bottle, half filled with water and cloſe ſhut up, till they died in it; then I examined the Air ſtanding above the water; it had no more a peculiar ſmell than the water;

it was ſomewhat increaſed to all appearance, and it extinguiſhed the flame of a candle. It ſeems theſe animals live chiefly on the phlogiſton in the empyreal Air, and perhaps too on the heat. I kept them alive during two years in water; and what is more, in the ſame water, with a crape covering the mouth of the bottle.—I have a ready method of finding out *whether the water contains empyreal Air or not:* I take for inſtance, one ounce of the water, and let fall into it about four drops of a ſolution of vitriol of iron, and add two drops of a ſolution of ſalt of tartar ſomewhat diluted with water. It yields immediately a dark green precipitate; which in a couple of miminutes changes into *yellow*, if the water contain empyreal Air; But if the water be previouſly boiled and grown cold without the acceſs of free Air, or if the water be diſtilled lately, the precipitate retains its green colour and does not turn yellow, unleſs about an hour after; and never changes into yellow, if it be kept in full glaſſes ſo that the free Air can have no acceſs to it. That the green precipitate of iron has its colour from the phlogiſton ſtill adhering to the earth, I before proved; (No. 15.) and hence it follows, that empyreal Air, though not in the elaſtic ſtate, is capable of attracting phlogiſton.--Another Experiment alſo proved, that aquatic animals attract empyreal Air from the water. I put a leech in a glaſs entirely filled with water, and precluded all acceſs of Air: In two days it was nearly dead; I then examined the water in the manner deſcribed before, and found that the iron earth retained its *green* colour.—The ſwelling of peaſe in *cold* water, is chiefly to be aſcribed to the empyreal Air contained in water. Filling a glaſs with water and putting ſome peaſe into it, four-and-twenty hours after this the water contains aërial acid, but no empyreal Air. In *boiled* water grown cold, the peaſe ſwell but very little.—I diſcover here the cauſe why waters diſtilled from vegetables not only loſe their ſmell, but depoſit at the bottom a mucilaginous ſubſtance, whenever the bottle containing them is often unſtopped; whereas in quite full and well-ſtopped glaſſes, theſe diſtilled waters preſerve conſtantly their ſmell and their limpid purity. All vegetables communicate to water ſomething mucilaginous, which goes over in diſtillation. The empyreal Air is the chief cauſe of this corruption; if this empyreal Air again enters the water, it attracts the inflammable from the ſubtle, oily, and mucilaginous ſubſtance, and changes the whole water.

95.

Heat is an integrant Part of ſeveral Bodies.

From what I have ſaid, (No. 93.) and the Experiments preceding it, I imagine that it is coming very near to truth to ſuppoſe that empyreal Air is a dulcified elaſtic fluid; a ſubtle acid united with ſome phlogiſton, and which by aſſuming more or leſs inflammable parts, acquires new properties.

Heat therefore is a peculiar acid, which has admitted a certain quantity of phlogiſton in its mixture; it conſequently muſt naturally unite with ſubſtances having an attraction either of acids or phlogiſton.— Such new-compounded bodies muſt preſent phænomena, which may be illuſtrated from the heat which is in union with them. Alkalies, abſorbent earths, and metallic earths, are thoſe bodies which really form an union with heat; and of conſequence form peculiar kinds of *neutral* ſalts. Theſe bodies muſt immediately part with heat (that ſubtle ſulphureous ſubſtance) whenever they unite with another ſubſtance, having a ſtronger attraction to them. All kinds of acids, even aërial acid, and ſometimes water alone, can cauſe this decompoſition; according as heat is more or leſs ſtrongly united with them.

Take neutral ſalts of that kind, which will part with their acid by heat only; (for inſtance, fixed alkalies, calcareous ſpar, magneſia alba, metallic earths, lime, and magneſia diſſolved in acid of nitre, magneſia diſſolved in marine acid, &c.) Calcine them either in open or cloſe veſſels half an hour or one hour with a radiant red heat: After they are grown cold, ſhut them immediately up in ſmall well-cloſed veſſels. All theſe are the ſame terreous ſubſtances which they originally were, excepting that inſtead of nitrous, marine, and aërial acid, they have adopted heat, with which they are united. Some can abſorb more heat, ſome leſs; and they acquire different properties in proportion to heat abſorbed; in the very ſame manner that happens to ſeveral kinds of earthy ſubſtances, capable of attracting more or leſs of aërial or other acids.—Thoſe which have attracted the moſt heat, acquire not only the quality of diſſolving in water, but alſo of parting with the ſuperfluous heat, by means of water only. (This phænomenon, as a ſubtle acid, heat has in common with many other acids, viz. the phoſphoric, arſenical, ſpatoſe or fluor, and aërial acids; which when perfectly ſaturated with earths, produce ſalts which are not ſoluble in water; but become ſoluble by the addition of more acid.) Such are likewiſe the fixed alkalies, quick-lime, and the earth of heavy ſpar *; theſe

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* The heavy or ponderous ſpar has a peculiar ſpecific gravity, which is to that of water as 4,500 to 1000: for which reaſon it was ſuſpected to contain ſomething metallic; but hitherto none have been able to extract any metal from it. However it obtained hence the name of *Marmor metallicum.* See *Cronſtedt's Mineralogy* Sect. xviii. and *Muria phoſphorea.* Linn. Syſt. Nat. iii. p. 99. (for the *Bononian Stone*, which has the

theſe become ſoluble in water by means of the heat united to them, and the redundant heat is expelled by the water; and they therefore grow hot with water, though this heat has not been ſenſibly obſerved exiſting in the ponderous earth.—Lime may be precipitated in the form of lime from the water; if alcohol or ſpirit of wine be poured in lime-water, the lime falls and may

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the quality of ſhining in the dark, after being heated is a ſpecies of this *Marmor metallicum*:)—but Meſſ. *J. G. Gahn* and *C. W. Scheele*, found at laſt, by a number of Experiments, that though the great and excellent Chemiſt Mr. *Marggraff* had ſaid, that the earth of this ſtone was calcareous, it was nevertheleſs moſt certainly an earth *ſui generis*; though having, no doubt, many properties in common with calcareous ſubſtances, *e. g.* they both efferveſce in acids, fire expels from both aërial acid, and then makes them ſoluble in water; they both depoſit a pellicle in the water, communicate cauſticity to alkalies, and diſſolve ſulphur, &c. But this ponderous earth is at the ſame time vaſtly different from all calcareous earths and gypſeous ſubſtances, for gyſpum is ſoluble in water; but this ponderous earth with acid of vitriol forms the heavy or ponderous ſpar, which is not ſoluble in water: So that if our ponderous earth be calcined, it diſſolves in water like lime; but on adding a ſolution of gypſum to it, the laſt ſolution is decompounded, and the vitriolic acid of the gypſum unites with the ponderous may be again diſſolved in water without a ſecond calcination; whence it appears, that it is ſtill united with heat as its menſtruum or ſolvent. It is therefore a ſalt which is inſoluble in ſpirit of wine, and conſequently quick-lime is never heated in ſpirit of wine.—If an acid be added to the abovementioned calcined earthy ſubſtances, they are diſſolved and violently heated, ſince

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ponderous earth, and forms again a heavy or ponderous ſpar, not ſoluble in water. This earth forms with the nitrous and marine acids, neutral ſalts ſhooting into cryſtals, which are not eaſily ſoluble; lime in the nitrous and marine acids yields a deliqueſcent ſalt. Lime and vinegar yield a cryſtalline ſalt; but ponderous earth and vinegar a deliqueſcent neutraliſed mixture.--In order to obtain this ponderous earth pure from the ponderous ſpar containing it, the ſpar muſt be treated with oils and coal-duſt in fire, in order to volatiliſe the vitriolic acid, which with phlogiſton forms a ſulphureous phlogiſticated vitriolic ſpirit and ſulphur, and at laſt leaves the ponderous earth by itſelf behind; but this is always a very tedious proceſs. To purify this ponderous earth it muſt be diſſolved in nitrous acid, and ſome fixed alkali neutraliſed with aërial acid or fixed Air be added to the ſolution, when by a double attraction the aërial acid attracts the ponderous earth, and precipitates, and the fixed alkali remains diſſolved in the nitrous acid. The precipitate waſhed and dried is *ponderous earth*, with aërial acid or fixed Air, which may be expelled by fire.

theſe acids decompound the former neutral ſalt, and expel the heat. If a thermometer be put into lime-water, and you add ſome water ſaturated with aërial acid, the ſpirit in the thermometer is obſerved to riſe a little.--If the acids poured on the calcined earths or alkalies be previouſly united with abſorbent earths, there enſues no heat, though the heat is actually expelled, but a double decompoſition takes place. It is well known, that from a mixture of a ſolution of *fixed ſal ammoniac*, and of a very cauſtic alkali, a true lime is regenerated. In this caſe the heat unites with the calcareous earth, and the marine acid with the alkali: if you pour an acid to this calcareous earth, the heat becomes immediately ſenſible. If calcareous earth contains more heat than other earths, which tho' ſtrongly calcined, will not diſſolve in water, the conſequence muſt be, that in caſe for inſtance, Epſom ſalt is decompounded with *lime milk*, the vitriolic acid moſt certainly unites with the calcareous earth, and the heat with magneſia; but ſince lime can attract more heat than magneſia, the redundant part of it muſt remain in the water.

ter. I put a thermometer into milk of lime, and left it ſtanding in the liquor for the ſpace of one hour; then I mixed with it ſome part of a ſolution of Epſom ſalt; immediately the ſpirit began to riſe.—Metallic earths, though not ſoluble in water, muſt have attracted a conſiderable quantity of heat. This becomes evident, from the great increaſe in weight acquired during calcination. They have either attracted empyreal Air during their calcination by means of their phlogiſton, and thus formed heat; or they have communicated their phlogiſton to the Air, and attracted heat from the fire; but either way there is empyreal Air in theſe calxes, which makes the overplus of their weight.—I ſaid theſe metallic earths muſt attract a conſiderable quantity of heat, for lime may be made ſoluble in water by their ſurplus. I ground freſh calcined litharge very fine, and poured on it a ſolution of fixed ſal ammoniac diluted with water; I kept the whole mixture in a well-ſtopped phial, and ſhook it frequently; after ſome hours the ſolution had depoſited calcareous earth, which made good lime-water, and was decompounding itſelf in the open Air, and fit for precipitating corroſive ſublimate with a yellow colour. If the ſolution of common ſalt be digeſted with litharge, a cauſtic mineral alkali is produced.—Subſtances which are united with a redundant heat, as alkalies,) quick-lime, and litharge, attract the inflammable which is found in oily mixtures: They diſſolve the fat oils and ſulphur, and yield with it ſoaps. If you pour an acid to a ſaponaceous ſolution, the acid unites with the alkali, and ſets the heat free; and ſince it becomes not ſenſible, it muſt undergo ſome new union; and meeting for that purpoſe with the oil, it unites with it; And the oil acquires by it the peculiar quality of becoming ſoluble in conſiderable quantity in ſpirit of wine, and to form with volatile ſpirits of ſal ammoniac prepared with quick-lime a peculiar kind of ſoap: Qualities which the fat oils likewiſe acquire after being diſtilled ſeveral times, when they likewiſe muſt have attracted heat from the fire. It likewiſe inſinuates itſelf into the pores of ſome ſalts, (*viz.* vitriol calcined to whiteneſs, fixed ſalt ammoniac, *terra* *foliata tartari,* &c.) But water again expels this heat.

Concentrated vitriolic acid, and acid of phoſphorus attract likewiſe a conſiderable quantity of heat, on account of their fixity; and though the other mineral acids, for want of fixity are incapable of attracting heat from the fire, they are nevertheleſs capable of abſorbing heat in great quantities. —In this reſpect they are ſimilar to volatile alkali, for when ſal ammoniac is diſtilled with a cauſtic fixed alkali or quicklime, the heat of their cauſtic ſalt unites with the volatile alkali, and the acid of ſal ammoniac with the lime. If to this volatile alkali an acid be added, the heat is ſenſibly expelled. —The weak mineral acids have the ſame qualities. Pour oil of vitriol on common ſalt in a ſmall retort, lute the neck of it to a receiver containing ſome water, this water will be heated without fire: for the vitriolic acid unites with the alkali of common ſalt, by which its heat is ſet free, and is immediately united with the marine acid; and is again expelled, when the water in the receiver is united with this acid. Hence the phænomenon reputed to be very extraordinary may be illuſtrated, viz. that oil of vitriol poured on common ſalt has the appearance of boiling, and is however cold; whereas the vapours grow hot in the Air. I am perſuaded this heat is no new production, but ariſes from the moiſture which is found in the Air: the boiling is nothing peculiar, for marine acid is always elaſtic in its dry ſtate: fuming ſpirit of nitre likewiſe is heated in the Air, and with water.—It is remarkable that heat expels ſome acids, and again that theſe acids in their turn expel heat: There are more ſuch affinities known in chemiſtry, where heat even inverts them; and perhaps the following Experiment will illuſtrate this matter. I filled a phial with aërial acid, and put ſome ground freſh calcined lime into it; then I ſtopt it up, turned it well, and put it inverted in a veſſel with oil: after eight days I opened the phial thus inverted under water, when I found to my aſtoniſhment, that no water mounted into the glaſs; but when a little water came to it, the Air was immediately abſorbed. Do not perhaps ſuch ſalts firſt part with their water by means of the heat, and then the dry acid have a weaker attraction to abſorbent ſubſtances than to heat?—From all theſe arguments it appears *how difficult it is to obtain a pure acid and a pure earth*; and I ſay not too much by aſſerting, that nobody ever ſaw a pure earth or a pure alkali.

96.

Inflammable Air.

If heat be a ſubtle acid, it ſhould be able to be united with more or leſs phlogiſton; and though all acids have not that property to attract phlogiſton in quantity, the greater part of them are however capable of attracting it in the greateſt abundance. To this laſt claſs belongs heat; for with a ſmall addition of phlogiſton it produces light; and with ſtill more, the well known inflammable Air.—I will begin with the metals: *Iron* conſiſts of an earth *ſui generis*, united with a certain quantity of phlogiſton, and a certain quantity of heat; all metals agree in that point; the difference is in their earths, which according to their nature have abſorbed either more or leſs phlogiſton. Whether heat be really an integrant part of metals, or whether the metallic pores are only filled with it, is of no conſequence, let it ſuffice that it is found in metals : the more phlogiſton a metal contains, the more heat is likewiſe found with it : no metal is diſſolved in acids, but it is done according to the laws of a double attraction. The acids unite with their earths, and the diſcharged phlogiſton with the ſame acids: if the latter have no attraction to the phlogiſton it is attracted by the Air; if this is wanting, it unites with heat, which in the ſame moment is diſcharged from the metals by the acids; then appear phænomena, which are peculiar to theſe unions. When the *vitriolic* acid diluted with water, comes in contact with the iron, it firſt unites with its earth; and ſince this weak acid has no conſiderable attraction to phlogiſton, and Air cannot come in contact with the iron where the acid is, the phlogiſton has no other way open but that of uniting with the heat of the iron; and thus the inflammable Air is compounded. The heat which ariſes from that ſolution, is only that which the phlogiſton cannot come into perfect contact with; but if the phlogiſton can form an union with another ſubſtance, the heat muſt increaſe in that caſe, ſince it is ſet at liberty: This happens when *nitrous* acid is poured on iron filings. Since *marine* acid does not much attract the phlogiſton, this acid acts in the ſame manner as the vitriolic.—Tin and zinc produce the ſame phænomena with theſe acids.

Inflammable Air cannot be deſtroyed by nitrous acid. I filled a phial with this Air, and poured on it ſome fuming nitrous acid; the acid did not grow red, nor was the Air abſorbed, but after ſome days was ſtill inflammable as before.

I give this Experiment here with a view to convince my readers that inflammable Air is not contained quite ready-made in the metals; for then it would follow, that even nitrous acid might ſeparate inflammable Air from metals, juſt as aërial acid is ſeparated from chalk: but ſince inflammable Air is compoſed of heat and phlogiſton, it is no wonder that this Air when its phlogiſton unites with the empyreal Air, ſeems entirely to diſappear with the empyreal Air, without leaving the leaſt indication of aërial acid or any thing elſe (No. 19, 46.). Pure water only can produce inflammable Air from iron: it is ſcum which conſtantly appears on the ſurface of the water, after it has ſtood over iron filings during ſome weeks, and has been ſomewhat ſtirred. If iron filings be diſtilled with ſalt ammoniac, and a bladder is tied to the mouth of the retort, inflammable Air is thus procured; for what ſhould become of the phlogiſton contained in the iron, after the marine acid has been united with the earth of iron, the volatile alkali having no connection or union with it?---Should any one ſuppoſe that the acids contribute ſomething to the generation of inflammable Air, I can readily clear his doubts: Mix zinc with cauſtic fixed alkali, and diſtill it in a glaſs retort; then the alkali attacks the earth of zinc, and in the bladder tied to the mouth inflammable Air is produced. Zinc digeſted with ſpirit of ſalt ammoniac, generates likewiſe inflammable Air.

If the cauſtic fixed alkali be compounded of heat and pure alkali, it is eaſy to conceive that if an inflammable ſubſtance be added, and the alkali attracts the acid united with the phlogiſton in the combuſtible body ſtronger than the acid does the phlogiſton, a double decompoſition muſt neceſſarily enſue; and the heat of the alkali muſt unite with the phlogiſton in an inflammable Air. With ſulphur this decompoſition cannot be produced; ſince its acid attracts the phlogiſton ſtronger than to allow the alkali to diſpoſſeſs the vitriolic acid of its phlogiſton.—The charcoal is moſt proper for the purpoſe, ſince it is a ſulphur compounded of phlogiſton and aërial acid. If coals be ground together with alkali, made cauſtic by quick-lime or fire and then diſtilled in a glaſs retort in an open fire, a great quantity of inflammable Air is thus obtained, containing no aërial acid: if a bladder be tied to the mouth, the alkali on the other hand loſes its cauſticity and effervesſces with acids.—In this Experiment the cauſe is found, why burning red-hot charcoal in a ſtove appears with a blue flame. Who would believe it that this flame is owing to the oil remaining in the coals? who would believe it, that this oil ſhould not be long ago expelled by heat? I filled a ſmall retort half full with very dry pounded charcoal, and tied to it a bladder emptied of Air. As ſoon as the retort became red-hot, the bladder was expanded; and when it was red-hot at the bottom, the bladder would no longer expand. I left the retort to cool, and the Air returned from the bladder into the coals. I again heated the retort, and the Air was again expelled; and when it was cooled, the Air was again abſorbed by the coals. This Air filled eight times the ſpace occupied by the coals. I heated the retort once more, the Air was expelled; and when left to cool, the coals re-abſorbed it. I repeated this Experiment very often, with the ſame ſucceſs. This Air puts out the flame of a candle, and contains a ſmall part of aërial acid —I tied to the mouth a bladder full of freſh Air, and after the foul Air was expelled by heat, and when the retort was ſtill red-hot at the bottom, this freſh Air was abſorbed by the coals; and being expelled again by heat, it was changed into foul Air. I found that charcoal attracted a ſtill greater bulk of aërial acid.—Wheat burnt to coals, and hartſhorn reduced to coals, do not yield ſuch Air.—But if charcoal, after it ceaſes yielding any more Air into the bladder, be heated by a ſtill more violent fire, till they be thoroughly *red-hot*, then another kind of Air is produced: I continued the fire till the bladder would expand no more, and then I let it cool: Part of the Air returned into the retort; a good part however ſtill remained in the bladder. This Air expelled by a violent red heat is inflammable Air. I again increaſed the fire on the ſame charcoals, but did not get any more Air, than what during their cooling might have been re-abſorbed by them; which amounted to eight times their bulk. I ſhook them out of the retort, and let them burn on in the open Air; then let them cool again and diſtilled them as before. I obtained at firſt outſet, before the retort was red-hot, an Air ſimilar to the foul; after the coals became red-hot, I obtained a great portion of inflammable Air. I left it again to cool, and repeated the fire with ſo violent a heat, that the retort began to melt at the bottom; but it yielded very little Air. Charcoals therefore muſt previouſly be kindled to a red heat in the open Air, before they will yield inflammable Air in the retort.---Charcoals contain both alkali and lime. Conſequently there are two attractions which deſtroy them. The releaſed alkali or the lime, uniting with the aërial acid and the heat penetrating through the retort with the phlogiſton. After the alkali has been ſaturated with aërial acid, it is impoſſible that more inflammable Air ſhould be generated; but if the coal be burnt in the open Air, ſome alkali may reappear; and then a new portion of inflammable Air may be compounded in the retort.---If charred hartſhorn be diſtilled in a violent fire, a great quantity of inflammable Air is generated for the ſame reaſon.

The flame of coals is produced, when the heat which is between the red-hot coals unites with the phlogiſton, and ſome part of the aërial acid with the aſhes: it cannot kindle immediately, ſince the empyreal Air which is between the coals, is already ſaturated with phlogiſton from the charcoals; it therefore riſes, and this Air meets the common Air: Theſe red-hot coals, therefore, if they have large intervals between them, muſt look as if they were burning.

It is remarkable that phlogiſton, which expands the empyreal Air to a certain bulk very thinly, (as may be obſerved in heat and light,)

light,) becomes by the addition of more phlogiſton ſo coarſe, that it may be kept in phials. Though in the inflammable Air produced from coal, no aërial acid, or at leaſt very little may be diſcovered; yet if this be ſeparated by means of the milk of lime, there however may be obſerved a good portion of ſuch aërial acid, after it be burnt. Hence it appears, that this inflammable Air has volatiliſed ſome part of the coal: In the ſame manner as arſenical acid during a digeſtion with zinc, generates inflammable Air, which contains ſome of the reguline arſenic. Does this volatiliſed ſubtle coal mix with the maſs of blood, and become the cauſe of the vapour of coals being noxious?

97.

Stinking ſulphureous Air.

1. I mixed the moſt ſubtle powder of quick-lime with an equal quantity of ſulphur, and made the mixture red-hot in a ſmall glaſs retort, to which I had tied a bladder emptied of Air. Some ſulphur

roſe into the neck of the retort, but no Air appeared. I poured marine acid on this liver of ſulphur; it cauſed a violent effervescence, and yielded a ſtrong hepatic ſmell. The mixture grew only moderately warm.

2. I mixed well ground manganeſe, (Molybdænum Magneſia. Linn. Manganeſe Cronſtedt's Mineralogy, Sect. cxiii.) with an equal quantity of ſulphur, and made it red-hot in a retort, with a bladder tied to it. The exceſs of ſulphur was ſublimed, and in the bladder I found a volatile ſpirit of ſulphur. The *reſiduum* had a greeniſh colour, it cauſed a ſcum with acids, and had an hepatic ſmell.

3. I prepared a cauſtic alkali from tartar and lime, which grew very hot with acids, but did not effervesce; and I melted it in a crucible provided with a cover, into a liver of ſulphur, which ſtrongly effervesced with acids, but was very little heated.

4. I collected the Air of theſe fuſions each ſeparately in a bladder: it had the following properties: (*a*) It did not precipitate lime-water; (*b*) it was in conſiderable quantities abſorbed by water, which thence

acquired a ſtrong hepatic ſmell and a ſweetiſh taſte; *(c)* and a burning candle introduced into a glaſs of it, was immediately extinguiſhed. If a third part of a glaſs veſſel be filled with this Air, and the reſt be common Air, this ſtinking Air kindles when a burning candle is put into it, and the glaſs is filled with a white thick vapour; this Air then ſmells like volatile ſpirit of ſulphur, and a white powder is depoſited which conſiſts of ſulphur.

5. I mixed charcoal-duſt with ſulphur, and diſtilled it in a retort with an emptied bladder tied to it, Firſt, I procured foul Air from it; then I increaſed the Fire and tied a new bladder to it; when I obtained a ſtinking ſulphureous Air, in all reſpects like the preceding.---In order to determine whether heat alone would produce Air from ſulphur like that which heat with phlogiſton generates with inflammable Air, I put a piece of ſulphur in a retort, with a bladder tied to it, and kept the ſulphur ſtrongly boiling during the ſpace of half an hour. The air in the retort was neither increaſed nor diminiſhed, and was changed into foul Air, but not into ſtinking ſulphureous Air.

The phlogiſton of coals muſt therefore contribute ſomething to this phænomenon.

6. I filled a retort with inflammable Air from iron, I put ſome ſulphur into it, and kept it boiling as before. In this and the preceeding Experiment I placed the retort in ſuch a manner, that the ſublimed ſulphur from the neck fell back and was again melted by the heat. The Air in this retort was, when it was grown cold, ſtinking, but did not diſſolve in water. It ſeems that this inflammable Air was overcharged with phlogiſton, which hinders the ſolution.

7. This inflammable ſulphureous Air ſeems to be a compound of heat, phlogiſton and ſulphur.

I filled a phial with this Air, poured ſome fuming ſpirit of nitre into it, and ſhut it carefully with a cork. The glaſs was inſtantly filled with red thick vapours; half an hour after this I inverted the phial, and opened the cork under water; the water inſtantly roſe into the glaſs, and filled it about three-fourths of the whole, and the water was ſomewhat mixed with ſulphur.

8. I repeated the ſame Experiment: (*i. e.* I filled a phial with the ſame Air) and let ſome drops of fuming nitrous acid fall into it, that I might put the ball of a thermometer into it. Inſtantly the ſpirit roſe in the thermometer; and a yellow powder was precipitated, which I found to be ſulphur.

9. From (No. 4.) it appears, that after the Air was burnt, a ſulphur was precipitated; which muſt be cauſed by that volatile ſpirit of ſulphur, which was ſeparated from that part of the ſulphur, which had been kindled.

I filled a phial with this ſtinking ſulphureous Air, and poured into it ſome volatile ſpirit of ſulphur, and ſhut it carefully. In half an hour's time the phial was covered with a yellow coating of ſulphur, and the Air was for the greateſt part abſorbed. I repeated the ſame Experiment, and introduced a thermometer into it, when I obſerved the ſpirit in the thermometer riſing conſiderably.

I poured ſome acid of arſenic into this ſulphureous Air. The acid became tinged yellow, and a true orpiment was precipitated.

tated.—Dephlogiſticated Spiritus Salis abſorbs likewiſe this Air, and the ſulphur is precipitated: But marine acid itſelf does not change it.

I am therefore of opinion, that if from this Air the phlogiſton be ſeparated, (which is the *medium* uniting ſulphur and heat,) this kind of Air is entirely decompounded: the heat is ſeparated, and the ſulphur is precipitated. Hence it likewiſe appears, that the volatile ſpirit of ſulphur has an attraction to phlogiſton.

Since neither the alkalies nor lime diſſolve ſulphur, unleſs they be made cauſtic, it follows, that the heat which is contained in theſe ſalts, muſt be the chief cauſe of their coheſion with ſulphur. If therefore an acid, for inſtance, the marine acid be added, it unites with the calcareous earth or the alkali, and the heat is then ſet free; and ſince it is not ſenſibly felt, it muſt enter into a new union; but not being able to unite with the ſulphur likewiſe ſet at liberty, unleſs there be added a greater quantity of phlogiſton, (No. 5.) this heat attracts the phlogiſton from a part of the ſulphur, and unites then with ſo much ſulphur as is required for the formation of ſtinking ſulphureous Air.

This opinion becomes more confirmed by the plaiſter of Paris, or vitriolated tartar, and volatile ſpirit of vitriol, which is conſtantly found to be contained in the lixivium after the ſulphur is precipitated; which is a neceſſary conſequence, after part of the ſulphur had been decompounded. If you pour to a ſolution of ſulphur in alkali, a great deal of acid at once, leſs ſtinking Air is generated, and you may obſerve in this mixture a kind of thin oil: however the oil remains not fluid always, but grows thick and hard in the open Air. It ſeems that the exceſs of acid immediately ſeizes upon the alkali; and ſince in that caſe no decompoſition, or at leaſt a very partial one, of the ſulphur is poſſible, the heat obtains too ſmall a quantity of phlogiſton to expand the heavy ſulphur into an airy vapour; a beginning only being made by forming an oil.

The generation of ſuch a ſulphureous Air, from fatneſs and ſulphur muſt have the ſame cauſe. I diſtilled in a retort a mixture of oil of olives and ſulphur, and tied a bladder to it: As ſoon as it began to boil, the bladder was expanded, and a ſtinking ſulphureous Air was obtained: Here being ſulphur, phlogiſton, and heat together, it is no wonder that this kind of Air ſhould be compoſed.—If any body wants to make Experiments with this kind of Air, he may uſe the following as the beſt method: Melt three ounces of ſmall iron filings with two ounces of ſulphur in a retort, the heat muſt be kept, till the ſulphur no more riſes into the neck of the retort; after all has been cooled, the retort may be broken; you will find the iron increaſed one ounce in weight. This ſulphurated iron diſſolves with ſtrong effervescence in acids; and nothing but ſtinking ſulphureous Air is obtained, without any *reſiduum* of ſulphur being left.—During the melting the exceſs of phlogiſton is ſeparated from the iron, and united with the heat of the fire; this is the cauſe of the appearance of light. (No. 81.) The remaining phlogiſton is that proportion of it which unites with the heat of iron and with the ſulphur diſengaged by means of the vitriolic acid, and this generates ſtinking ſulphureous Air. (No. 6.)

THE END.

NOTES
ON THE
PRECEDING WORK.

BY

RICHARD KIRWAN, Eſq. F.R.S.

WITH A

LETTER

TO HIM FROM

JOSEPH PRIESTLEY, L.L.D. F.R.S.

IT may be ſaid, with equal truth, of the progreſs through ſcience, as of that through ſpace, that it is impoſſible to arrive at any diſtant point without paſſing through the intermediate, however great we may ſuppoſe the energy of motion in the one

caſe,

caſe, or that of genius in the other. The great Newton could never have arrived at the diſcovery of the Mundane ſyſtem, if he had not availed himſelf of the previous diſcoveries of Galilee and Kepler. Many inſtances of this ſort occur in the Hiſtory of the Sciences, nor is there any thing that ought to encourage us more in our purſuits of natural knowledge, than the chance that perhaps one ſtep further may be a capital diſcovery, and the certainty, that if it be not, it at leaſt neceſſarily leads towards one. There never was perhaps a more ſignal proof of the incapacity of the human underſtanding to make any important diſcovery, without paſſing through the intermediate ſteps, than the attempt of our Author to penetrate into the very eſſence of fire, without availing himſelf, or perhaps being acquainted with the intermediate diſcoveries of Fahrenheit, Black, and Crawford. Thoſe of the laſt he was certainly unacquainted with, Dr. Crawford's Treatiſe being much poſterior to his; thoſe of the former, he ſeems not to have attended to; yet, without a previous knowledge of theſe, his genius, great as it confeſſedly is, and his chemical abilities, equal, if not ſuperior, to thoſe of any of his cotemporaries, could not ſupply the deficiency. With regard to Air, (the knowledge of which indeed was not his principal object) he has done little elſe than repeat, with ſome variation in the manner, the Experiments publiſhed ſome years before his Treatiſe, by Dr. Prieſtley, to whom, indeed, the Doctrine of Air owes more, notwithſtanding he cultivated moſt branches of it without a guide, and paſſed through all the intermediate ſteps by himſelf, than to any one who has yet appeared, though it has been more aſſiduouſly cultivated theſe eight years paſt by the moſt eminent Philoſophers, both at home, and on the Continent, than any other in the whole circle of experimental knowledge.

Page 7. § 8.

THIS Experiment was alſo made by Dr. Prieſtley, but he did not find the diminution of the common Air quite ſo great, being only one-fifth; whereas Mr. Scheele found it to be nearly one-third; probably Dr. Prieſtley did not expoſe theſe ſubſtances as long to each other as Mr. Scheele has done, but he applied the teſt of nitrous Air, which Mr. Scheele neglected to do in this and every other caſe; however, he has aſcertained that the greateſt diminution does not reach quite to one-third. Mr. Scheele thinks that ſix ounce meaſures of Air were loſt in this Experiment; but as he remarked, that part of the ſulphur was precipitated, (an effect which is always produced by the addition of an acid) he ſhould rather have judged that either part of the Air was converted into an acid, or that an acid was precipitated from the Air by the very act of phlogiſtication, as Dr. Prieſtley has done.—In this caſe 10, 89 cubic inches (which according to Mr. Fontana, are equal to 5, 86 grains) of fixed Air were precipitated.

Page 9. § 10.

In this caſe, the ſulphur was not precipitated, becauſe its quantity was very ſmall relatively to the quantity of cauſtic alkali to which it was united, and which was much greater than was neceſſary to keep it in ſolution; hence the fixed Air uniting to this ſuperfluous proportion of fixed alkali, no precipitation enſued, it even appears by the liquor's becoming colourleſs, that the ſulphur was decompoſed; there were only thirty grains of ſulphur in this ſolution, whereas in the firſt Experiment there were four hundred and eighty.

Page 10. § 11.

Four ounces of a ſolution of ſulphur in lime-water, can contain but very little ſulphur, for boiling water diſſolves only about one three-hundredth part of its weight of lime, as Mr. Bergman affirms, and conſequently, four ounces in the temperature of fifty-nine degrees of Fahrenheit, cannot contain above ſix grains of it; now ſix grains of quick lime cannot take up above three grains of ſulphur, ſo that here there could not be more than three grains; hence this ſmall quantity was eaſily decompoſed, the phlogiſton of the ſulphur uniting to the Air, from whence fixed air was precipitated, and the vitriolic acid to the calcareous earth.---Our author did not find that the volatile acid of vitriol was altered in his Experiment, but Dr. Prieſtley diſcovered that not only common Air is phlogiſticated by this acid, but that the acid itſelf is altered, and real ſulphur formed of it in tubes hermetically ſealed, 4 Prieſtley 124.

Page 11. § 13, 14, and 15.

The effects of nitrous Air on bladders, are handled at large by Dr. Prieſtley, 1 Pr. 214.—3. Pr. 153.—4 Pr. 60. . . . . . . the diminution of common Air by eſſential oils was alſo firſt noticed by him, 3 Pr. 92. 4 Pr. 363. that the green and blue calces of iron diminiſhes common Air, has alſo been ſince found by Dr. Prieſtley, Vol. IV. p. 289.

Page 13. § 16.

In this ſection, Mr. Scheele endeavours to prove, that the diminution of common Air in the phlogiſtic proceſſes above deſcribed, ariſes from a total loſs or eſcape of that quantity by which it was diminiſhed; for, ſays he, it is not to be found in the ſhape of Air, the remaining Air weighing rather leſs than before, nor is it to be found in the remaining liquor, § 10. *For, ſays he, this alkaline lye being mixed with lime-water produced no precipitate.* It is ſurpriſing this accurate chemiſt did not obſerve the impoſſibility of the precipitation of lime-water in this inſtance; for the quantity of fixed Air precipitated muſt have been very ſmall; (but how ſmall cannot be determined, as he has not mentioned how much the common Air was diminiſhed): now it is well known that a large quantity of any ſubſtance being united to a very ſmall quantity of another ſubſtance, to which it bears any affinity, retains this latter moſt obſtinately, how then could the ſmall portion of lime contained in lime-water ſeparate a few grains of fixed Air, from about two ounces and a half of a cauſtic alkaline lye by bare affuſion, and without the aſſiſtance of heat, when Dr. Black, to ſtrip fixed alkalies of all their fixed Air, was obliged to mix them with one-third of their weight of quick lime in boiling water? It is no leſs extraordinary that he did not remark the precipitation of lime-water by phlogiſtic proceſſes ſo frequently mentioned by Dr. Prieſtley, the firſt volume of whoſe Experiments he muſt have ſeen.

Page 16. § 17.

Nine ounces of phoſphorus, occupied at moſt the ſpace of four ounces and a half of water; and ſubſtracting this ſpace from the capacity of the retort, we ſhall find it contained only twenty-five ounces and a half meaſures of Air, and by this Experiment, it would appear that theſe twenty-five ounces and a half meaſures were diminiſhed by the combuſtion of phoſphorus nine ounces, that is more than one third; a degree of diminution greater than has ever yet been produced in common Air, by any Experiment. Mr. Lavoiſier, who examined the phænomena attending the combuſtion of phoſphorus very carefully, never found common Air to be diminiſhed by it more than between one-fifth and one-ſixth, and even this diminution is much greater than that which is cauſed by the combuſtion of any other ſubſtance. But Mr. Lavoiſier has carried this matter much further; for, by a nice and careful Experiment, he found, that the phoſphoric acid thus formed, had acquired the very weight which the Air had loſt. Vol. I. p. 335; and, conſequently, that no part of the Air had been loſt, or had made its eſcape, but had ſimply been abſorbed by the phoſphoric acid; an Experiment which if Mr. Scheele had made, it would probably have ſpared him the pains of writing this treatiſe.

However, the greater diminution of Air by the combuſtion of phoſphorus, than by that of any other ſubſtance, is very remarkable; and certainly ariſes from its producing greater heat, during its combuſtion, than any other combuſtible; for

Dr. Hales has long ago remarked, that Air, in which a ſmall, or ill-lighted candle has ceaſed to burn, will yet admit a larger and better lighted candle to burn, but for a ſhorter time. Now the diſcoveries of Dr. Crawford prove, that the more phlogiſton is thrown into Air, in a given time, the more of its fire it parts with; which, if not taken up by the burning ſubſtance, becomes redundant, and cauſes ſenſible heat. On the other hand, it is well known, that phoſphorus imparts its phlogiſton to Air more readily than any other combuſtible; and I myſelf have found, that moſt acids are incapable of taking it up in any conſiderable degree; and hence it becomes redundant, and a great heat is produced, which promotes a ſtill further ſeparation of phlogiſton; a circumſtance which is always attended with a proportionable diminution of the Air that receives it.

Page 17. § 18.

The diminution of common Air, by phoſphorus, without combuſtion, has not been experienced by any one that I know of, except the very ingenious and induſtrious Mr. Fontana; and, if I miſtake not, he has found it much leſs conſiderable than our author, who it were to be wiſhed had informed us in what ſtate the phoſphorus exiſted after this diminution, and whether it had increaſed in weight.

Page 17. § 19.

In this Experiment, the aërial acid was depoſited in the water, or metallic ſolution, therefore it was a vain attempt to ſeek for it in the remaining Air.

Page 20. § 21.

Dr. Hales lighted a candle in a cloſe receiver by means of a burning glaſs, he alſo found that in proportion to the ſize of the veſſel, the Air was more diminiſhed in ſmall than in large veſſels, when candles of the ſame ſize were uſed; but in general theſe Experiments are liable to a good deal of uncertainty, not only from the different degrees of inflammability of the tallow or other combuſtibles uſed, according to their greater or leſſer purity, and from the quantity of inflammable Air unburned, which mixes with the reſiduum of common Air, but from another circumſtance firſt diſcovered by Mr. Fontana, viz. that the coaly ſubſtance of the combuſtible abſorbs very plentifully all ſorts of Air, ſo that it is hard to ſay how much Air is diminiſhed by the mere act of combuſtion.

Page 23. § 22.

This Experiment clearly proves that a mixture of fixed Air, renders common Air leſs fit to ſupport flame, as Mr. Cavendiſh had diſcovered eight years before; Dr. Hales had alſo found that the preſence of ſalt of tartar enables a candle to burn longer than it would otherwiſe by abſorbing ſome vapour, or rather the aërial acid; ſo that here the alkali acted the ſame part as the quick lime in Mr. Scheele's Experiment. 1 Hales, 271.

Page 25. § 23.

Dr. Prieſtley, who firſt made this Experiment, explains it ſo clearly that it is needleſs to ſay any thing further concerning it.

Page 27. § 25.

The obſervations that occur in this ſection, ſeem peculiarly to belong to Mr. Scheele: Dr. Prieſtley has ſince enlarged, and conſiderably improved them. See Vol. III. p. 243, and Vol. IV. p. 1. But to underſtand this matter perfectyl, it will be neceſſary to remark, 1ſt. That dephlogiſticated Air is one of the component parts of nitrous acid, let the other be what it may; (if indeed there be any other beſides dephlogiſticated Air in a concrete ſtate, and deprived of the quantity of elementary fire neceſſary to keep it in an aërial ſtate). This ſeems to me the manifeſt reſult of the numerous Experiments of Dr. Prieſtley, ſome of which have been repeated by Mr. Fontana and Lavoiſier.

For, by combining this acid with fixed alkalies, earths of any ſort, or various metallic ſubſtances, deplogiſticated Air has been obtained in great plenty. Now it ſeems to me much more reaſonable to attribute the origin of this Air to one and the ſame ſubſtance, which has been combined with all of them, than to ſo many ſubſtances eſſentially different from each other; moſt part of which are incapable of affording the leaſt particle of it, either alone or in combination with any other ſubſtance. 2d, That nitrous Air is produced by the ſolution of any ſubſtance in nitrous acid, as ſome phlogiſton is always diſengaged, and of this nitrous air a conſiderable quantity is reſorbed by the ſolution, as Mr. Fontana has particularly remarked. 3. That the colour of the nitrous acid and its vapour depends on the intimacy of the union of the phlogiſton they contain with the ſaline part; and, conſequently, in ſome meaſure, of the proportion of the aqueous part, and the degree of heat by which they are united; the union of the acid and phlogiſton being ſo much the more intimate, as the degree of heat in which they are united is greater, and as the proportion of water is leſſer; and hence red ſpirit of nitre, by the addition of more water, becomes yellow; and by the addition of ſtill more water, green; as the preſence of water, in proportion to its quantity, diminiſhes the force with which the acid and the phlogiſton attract each other; ſee Dr. Prieſtley's Obſervations, Vol. III. p. 198 and 243. And on the other hand, blue ſpirit of nitre is by heat converted into green, and the green into yellow, and this into red; but if the proportion of water be conſiderable, the former colours return on the ceſſation of heat, Vol. IV. p. 4.

Towards the end of the operation of diſtilling ſpirit of nitre, and indeed, during moſt part of the proceſs, dephlogiſticated Air is produced, as Dr. Prieſtley has ſhewn at large; and hence a candle will burn in the receiver with a vivid flame. The red vapour proceeds from the decompoſition of nitrous Air, by the dephlogiſticated Air: it is this Air that pervades the receiver filled with phlogiſticated Air, and enables it to permit a candle to burn in it.

He aſks from whence the phlogiſton proceeds at the end of the operation? I anſwer, from the fixed alkali of the nitre, which phlogiſticates the vitriolic acid after the nitrous is for the moſt part expelled, and hence the vitriolic acid is in part phlogiſticated in tartar vitriolate, as Mr. Baumé has ſhewn at large in the ſixth volume of the *Mem. Etrang. & Chymie Experim.* Vol. II. p. 199. but this vitriolic acid is again dephlogiſticated by the nitrous, which then is expelled, and forms the red vapours at the end of the operation.

Page 30. § 27.

a. Our author has no where proved that the ſaturation of nitrous acid with phlogiſton, produces real Fire, as is here aſſerted.

b. Common Air does not loſe its elaſticity by an union with nitrous Air, or the phlogiſton of this latter, as Dr. Hales originally conjectured, concurrently with all philoſophers, both at home and abroad, until Dr. Prieſtley clearly ſhewed that common Air by phlogiſtication was not only diminiſhed in *bulk* but in *maſs* alſo, and that the remaining Air was juſt as elaſtic as ever; which has lately been confirmed by Mr. Herbert, of Vienna. The red vapours which are ſeen on uniting nitrous Air with common Air ariſe from a change in the proportion of the nitrous acid and phlogiſton, relatively to each other, which produces a conſequent change in the degree of force with which they are united; for the colour of all bodies ſeems to depend on the degree of attraction with which their conſtituent parts are united to each other; as I have found in numerous inſtances. As ſoon as nitrous Air is decompoſed by common or dephlogiſticated Air, nitrous vapour is produced, which conſiſts entirely of nitrous acid and phlogiſton; but the ſmaller the proportion this latter is, the more forcibly it is attracted by the acid; and hence the purer the common or dephlogiſticated Air is, and conſequently the more phlogiſton it takes up, the leſs of this phlogiſton remains with the nitrous acid, and then the vapours are exceeding *red*, as Dr. Prieſtley obſerved, Vol. II. p. 41. When the proportion of phlogiſton remaining with the nitrous acid is ſomewhat greater then the force of their attractions is leſſer, and the vapours are of an *orange colour*, and when the proportion phlogiſton is ſtill greater, then the united ſubſtances are *colourleſs*, as is the caſe of that which is ſtrictly called *Nitrous Air*; the conſtituent principles of which attract each other ſo weakly, that they are eaſily decompoſed by all ſubſtances that attract only one of them as elementary Air, which attracts the phlogiſton only, and water, which attracts the acid only, as Mr. Fontana has ſhewn; ſee his Tract on Nitrous Air, p. 13. . . . . . The heat produced by the union of nitrous and common, or dephlogiſticated Air, ariſes from the ſeparation of elementary Fire from the latter Airs, at the time they take up the phlogiſton of the former, as Mr. Magellan aſſerts in conformity to Dr. Crawford's theory; the diminution ariſes from the precipitation of fixed Air from the common or dephlogiſticated Air.

c. What Mr. Scheele here calls a ſpecies of nitrous Air, is the ſame as what Dr. Prieſtley calls *nitrous vapour*; of whoſe phænomena he treats at large in his 3d and 4th volume of Obſervations, and the ſame as what Mr. Bergman very juſtly calls *Phlogiſticated Nitrous Acid.* Mr. Scheele firſt obſerved, that the nitrous acid in nitre is phlogiſticated by barely keeping nitre red-hot for ſome time, which ſhould ſhew him that fixed alkalies might furniſh phlogiſton in certain circumſtances.

Page 3. § 28.

I ſhall here endeavour to anſwer the queries of Mr. Scheele. Is not the peculiar kind of Air by the mediation of which Fire burns, and which conſtitutes the third part of common Air, introduced into the receiver by diſtillation? That is to ſay, is not the dephlogiſticated Air in the proceſs mentioned in the laſt ſection, produced by diſtillation? It certainly is; yet not in the manner in which Mr. Scheele would have it, viz. by the penetration of a compound of Air and phlogiſton (which according to him conſtitutes heat) through the retort or crucible, and a ſubſequent decompoſition of theſe two principles, for in no inſtance does it appear that phlogiſton penetrates through glaſs, much leſs a compound of Air and phlogiſton; and if Mr. Scheele's notion were true, then metallic calces, or at leaſt black manganeſe, would be reduced by bare heat; for this laſt calx dephlogiſticates nitrous acid, and therefore has a ſtronger affinity with phlogiſton than this acid as Mr. Bergman affirms, and therefore heat ought as well to be decompoſed by it as by nitrous acid, which Mr. Scheele will not certainly pretend.

But the truth is that the nitrous acid is itſelf partly decompoſed during the diſtillation of Glauber's ſpirit of nitre, an event analogous to ſeveral others; thus if nitre be diſtilled *per ſe* in glaſs veſſels, the greater part of the produce is dephlogiſticated Air, as Dr. Prieſtley firſt found, 2 Prieſtley, 89, and afterwards Mr. Fontana, who found the nitre totally alkaline after this operation, and ſcarce any trace of the nitrous acid in the receiver, ſee Rozier's Journal for November, 1778; an evident demonſtration that the dephlogiſticated Air proceeded from that acid, and not from the decompoſition of the matter of heat, as Mr. Sheele would have it. I paſs over ſeveral other inſtances of the decompoſition of the nitrous acid in the diſtillation of red precipitate, and even of nitrous acid and glaſs, and in various other Experiments to be met with in Dr. Prieſtley's Obſervations; as after what has been ſaid, all further proofs would be ſuperfluous.

Page 34. § 29.

The Experiment mentioned here has been repeated by that very accurate philoſopher, Mr. Fontana, but without ſucceſs; ſuppoſing it to be true, it would only prove a partial decompoſition of the nitrous acid.

Page 35. § 30.

The apparatus here mentioned by Mr. Scheele, is ingenious; but as he obſerves, very inadequate to the purpoſe of nice diſcrimination; nay, may often lead into conſiderable miſtakes, as muſt be very apparent to any one converſant in Dr. Prieſtley's works.

Pages 41, 43, 44. § 31, 32, 33.

Dr. Prieſtley has ſhewn in the 4th volume of his Obſervations on Air, p. 203, that manganeſe diſtilled *per ſe* yields dephlogiſticated Air; it is no wonder then it ſhould alſo yield it when diſtilled with acids; but the Doctor obſerves, it ſtill continues black and unchanged; whereas, according to our author, it ſhould have acquired phlogiſton.

Pages 44, 46, 47. § 34, 35, 36.

The Experiments here mentioned, prove nothing more than the decompoſition of the nitrous acid, and have all been firſt made by Dr. Prieſtley.

Page 48. § 37.

It is very extraordinary that Mr. Scheele, whoſe general theory is ſo very different from that of Mr. Fontana, ſhould nevertheleſs in this ſection hit upon exactly the ſame explanation of the reduction of the calces of what are called the *noble* metals, and which indeed appears to me very ſatisfactory, if we take in a circumſtance deducible from the diſcoveries of Dr. Crawford. It was obſerved by Mr. Fontana, in his excellent Treatiſe on dephlogiſticated Air, that metallic ſalts formed by the ſolution of mercury or ſilver, in the nitrous acid, contain ſome phlogiſticated nitrous acid, and a quantity of nitrous Air, which increaſes their abſolute weight; on the other hand, Dr. Crawford diſcovered that metallic calces (to which ſtate metals are reduced by ſolution) contain more elementary Fire than the metals from which they proceed. When therefore theſe metallic ſalts are heated to a certain degree, the nitrous acid is decompoſed as uſual, the metallic calces attract the phlogiſton contained in the nitrous acid, or nitrous Air, and are revivified; and the Air thus dephlogiſticated, attracts the elementary Fire in the calces, which is as neceſſary to the exiſtence of dephlogiſticated Air, as the phlogiſton is to the revivification of the calces; thus both phænomena are clearly explained. Mr. Scheele in the ſame manner attributed the revivification of theſe calces to a decompoſition of a compound of Air and phlogiſton, not pre-exiſting in the metallic ſalts, (which yet their increaſe of weight and abſorption of Air evidently demonſtrates) but introduced in the ſhape of heat through the pores of the retort; a ſuppoſition totally needleſs.

In all metallic ſolutions, the incumbent Air is in ſome meaſure phlogiſticated, and conſequently ſome fixed Air precipitated, which is abſorbed by the ſolution; leſs indeed when the nitrous acid is uſed, than when the vitriolic or marine are employed, becauſe the nitrous is ſo greedy of phlogiſton, that it takes it almoſt all up. The fixed Air thus formed, and alſo the phlogiſticated Air are in a great meaſure abſorbed by the ſolution. Thus Dr. Hales has obſerved, that the ſolutions of iron and copper in lemon juice, abſorb more Air than they produce.

Page 52. § 41.

The reſtoration of the acid of arſenic to the ſtate of white arſenic by mere heat, is a very curious diſcovery, entirely due to Mr. Scheele, to whom indeed we owe the complete knowledge of this mineral. He has clearly demonſtrated that it can exiſt in three ſtates; that of regulus, when it is completely phlogiſticated; that of a white calx, commonly called white arſenic, when it is in ſome meaſure dephlogiſticated; and that of an acid, when it is entirely dephlogiſticated; and thus removed all the obſcurities which the greateſt chemiſts had found in treating this ſemi-metal. But the Experiment here mentioned, proves no more than that during its dephlogiſtication, it had abſorbed ſome phlogiſticated Air, which is decompoſed during ſtrong heat, as above ſhewn. If the partial reſtoration, mentioned by Mr. Scheele, had been brought about in the manner he pretends, then might the calx itſelf be compleatly metallized in the ſame manner, which does not happen.

Page 54. § 42.

This Experiment of Mr. Scheeſe's is analogous to ſome of Dr. Prieſtley's and Mr. Fontana's, which prove the entire deſtructibility of dephlogiſticated Air, (which is here called empyreal Air) by phlogiſtic proceſſes, or rather its entire convertibility into aërial acid, that is, fixed Air. The reaſon why common Air cannot be ſo far diminiſhed is, becauſe it does not contain the ſame quantity of elementary Fire, as Dr. Crawford has ſhewn; for the hepar parts with its phlogiſton only in proportion as it receives elementary Fire from the Air to which it communicates phlogiſton; for here a double decompoſition takes place.

Page 54. § 43.

What Mr. Scheele here terms *foul* Air, is that which by Dr. Prieſtley has very juſtly called phlogiſticated Air; in which he has been followed by all other philoſophers; even Mr. Bergman, though he ſeems to have embraced the ſyſtem of Mr. Scheele, in a late tract adopts the ſame appellation, by which it would appear as if at preſent he had relinquiſhed that hypotheſis; and this Experiment, among others I could mention, ſeems to me a full proof that fixed Air conſiſts of elementary Air and phlogiſton, that phlogiſticated Air is for the moſt part an aërial ſulphur, conſiſting of fixed Air, ſuperſaturated with phlogiſton; and that in phlogiſtic proceſſes, fixed Air is generally generated, and not phlogiſticated Air; ſo that the phlogiſticated Air that remains after ſuch proceſſes pre-exiſted, and was not formed by thoſe proceſſes, but that the production of fixed Air is their genuine effect.

Page 57. § 47.

This Experiment alſo proves what was advanced in the foregoing remark, viz. that phlogiſticated Air is not produced by what are generally called phlogiſtic proceſſes, viz. the union of nitrous and elementary Air, the calcination and ſolution of metals, reſpiration, combuſtion, expoſition to ſulphureous and phlogiſtic fumes, &c. for a candle would burn in this reſiduum, which yet a very ſlight phlogiſtication would prevent it from doing. Dephlogiſticated Air and its properties were firſt diſcovered by Dr. Prieſtley, and its power of promoting combuſtion aſcertained; he alſo diſcovered in what combuſtion conſiſted, and how far elementary Air was neceſſary to it; and why Air that had once ſerved for combuſtion, could never ſerve again until it was purified; and the means of its purification; and what becomes of the phlogiſton after combuſtion. He ſhews that combuſtion conſiſts in the rapid ſeparation of phlogiſton from combuſtibles by Air; that the phlogiſton ſo ſeparated, unites to the Air; that aërial acid, or fixed Air is then depoſited; that Air after this is leſs ſuſceptible of taking up phlogiſton than before, and conſequently cannot again ſerve for combuſtion until it is purified, and that it is purified by agitation in water, or by the proceſs of vegetation; all which, to ſay nothing of his other theoretic diſcoveries, clearly ſhew how little he deſerves the appellation of a mere Experimenter, which he himſelf ſometimes modeſtly aſſumes, and which ſome others would fondly attribute to him. This indeed, I may confidently affirm, that without poſſeſſing the common *routine* of chemiſtry, which he every where owns, his writings have contributed more to its advancement and perfection, than all thoſe which have appeared ſince the days of Stahl;—there remained but one circumſtance more, the knowledge of which was neceſſary to complete the theory of combuſtion; namely, that of the appearance of light, and the ſource of the Fire that appears in that ſhape; and this knowledge has lately been happily ſupplied by Dr. Crawford, who clearly demonſtrates from Experiments, as ingenious as irrefragable, that the Fire then produced to view, proceeds from the Air, and not from the combuſtible ſubſtance; which, on the contrary, receives a great part of it, at the ſame time that it communicates phlogiſton; and that dephlogiſticated Air contains more than four times the quantity of Fire that common Air does; and hence, as well as from its ſuperior attraction to phlogiſton, promotes combuſtion more powerfully.

Page 59. § 50.

According to the very exact Experiments of Mr. Fontana, the thermometer of Fahrenheit at 55, and the barometer at 29 one-half, the weight of this bulk of dephlogiſticated Air ſhould be 15, 15 grains, and of common Air 13, 98 grains.

Page 61. § 51.

Liver of ſulphur is undoubtedly decompoſed by Air, and the decompoſition was here haſtened by increaſing its ſurface, and conſequently heat was produced by the phlogiſtication of the Air, as already explained.

Page 62. § 52.

Dr. Hales has long ago ſhewn that a mixture of filings of iron and water diminiſhes Air; and as iron is evidently dephlogiſticated and ruſted, this exactly coincides with the theory above-mentioned; the phlogiſton uniting with the Air.

Pages 67, 72, 74. § 56, 57, 58.

The queries in this fifty-ſixth ſection ſeem to me very little to the purpoſe, and the concluſions deduced from them altogether groundleſs.

a. Why is the breath inviſible at the diſtance of ſix feet from a Swediſh ſtove well heated, though viſible in a cooler atmoſphere in ſummer? But has he told us the heat that prevails at the diſtance of ſix feet from that ſtove? All I know is, that the breath is inviſible in the temperature of 63 degrees, and perhaps at a lower temperature.

b. Why does a candle burn without interruption, and its ſmoke aſcend perpendicularly at ſome diſtance from the heated ſtove? Becauſe the rarified Air that is not oppoſite the entrance of the ſtove, riſes to the cieling by the difference of its ſpecific gravity, and carries the ſmoke with it.

c. Heat is not a particular ſubſtance, diſtinct from things heated, except by heat, the *cauſe* of heat be meant, that is elementary Fire, lodged in bodies, and this cannot be carried up through the ſtove, unleſs the hot bodies can paſs through it; and here it is evident, that no hot Air can be carried up but that which is oppoſite the entrance.

e. This tremulous motion ariſes from the effluvia of the heated body, and preſents no ſort of difficulty. A Frenchman has lately imagined he ſaw the element of Fire aſcend in this manner, but his imaginations have convinced nobody, at leaſt in this country.

Page

Page 77. § 60.

The nature of light is as yet very far from being accurately aſcertained; but to me it ſeems very improbable, notwithſtanding the Experiment here alledged, that it is compounded of phlogiſton and Fire. Firſt, becauſe of the repulſive nature of Fire and phlogiſton, diſcovered by Dr. Crawford.

2dly, Becauſe I cannot find in any other inſtance that phlogiſton has the power of penetrating ſolid bodies.——3dly, Becauſe it would follow by theſe Experiments, that the calxes of the perfect metals, and alſo dephlogiſticated ſpirit of nitre poſſeſs a greater attraction to phlogiſton than the other conſtituent part of light does; and if ſo, I do not ſee why this ſpirit of nitre may not be totally phlogiſticated, and theſe metallic calxes intirely reduced by light. Now dephlogiſticated nitrous acid may be turned *green*, but never *red* or *brown* by expoſition to light, as I have experienced; nor is the marine dephlogiſticated acid in the leaſt altered by it, nor the calx of manganeſe, commonly called black manganeſe, in the leaſt diſcoloured, though it attracts phlogiſton more ſtrongly than the nitrous acid, and on account of its colour, can imbibe light very copiouſly.

If conjecture be allowed, I ſhould rather think that *light* conſiſted in a *rapid impulſe* of elementary Fire, as *heat* does in the *exceſs* of Fire, contained in the hot body over the quantity neceſſary to keep it in an equilibrium with thoſe bodies with whoſe temperature it is compared. This impulſe may poſſibly tend (by reaſon of the repulſion betwixt Fire and phlogiſton) to ſeparate and expel phlogiſton from the bodies that are expoſed to it, eſpecially when this expulſion is promoted by the attraction of a body to which phlogiſton hath a ſtrong affinity, as the calxes of the perfect metals and dephlogiſticated nitrous acid; and this effect, it may perform more readily than mere *heat* can, becauſe *heat* operates more ſlowly, and expands bodies in proportion to its quantity, which *light* does not, for moon-light collected in a focus, though exceſſively bright, does not affect a thermometer, as Muſchenbroek affirms; and this expanſion allows the elementary Fire ſufficient room without expelling the phlogiſton; beſides, let it be conſidered that the whole quantity of light that falls on any body, is never reflected; part of it is therefore imbibed, and the capacity of the body for containing it is increaſed, and conſequently the phlogiſton is in part expelled, by reaſon of their repulſive action on each other.

Page 78. § 61.

This calx of ſilver ſtill retained fixed Air, and alſo nitrous Air; the light, that is, elementary Fire uniting to theſe Airs, expelled the phlogiſton, which the calx then took up.

Page 79. § 62.

a. The red precipitate always contains a certain quantity of the nitrous acid, as Bayen has ſhewn; and hence the reduction may happen, as above explained.

b. The calx of gold contains nitrous or marine Air, and hence it is reduced as the above.

d. By pure nitrous acid, the author means the dephlogiſticated colourleſs nitrous acid. I have expoſed this in a phial half full, and cloſed with a glaſs ſtopper, to the ſolar light, and in a quarter of an hour found it phlogiſticated; but when the phial was quite full, this did not happen, though I expoſed it two hours to the ſame light. It appeared to me in the firſt caſe, that the vapour only was acted upon, being changed from white to red; this vapour was abſorbed by the liquor which then became greeniſh. In this caſe, the vapour may have taken phlogiſton from the Air, whoſe capacity for containing Fire was increaſed; this I am the more inclined to ſuſpect, becauſe the more of the bottle was left empty, the deeper green the liquor appeared to me to aſſume.

Pages 80, 82, 84. § 63, 64, 65.

All the reductions mentioned in theſe ſections are explicable on the principle above-mentioned, without having recourſe to any phlogiſton contained in light. Our Author's Experiments on Manganeſe are truly admirable. In his Memoirs, and thoſe of Mr. Gahn, to be ſound amongſt thoſe of the Academy of Stockholm, the nature of this ſemi-metal, which had baffled the efforts of the moſt ſkilful chemiſts before his time, is clearly ſet forth.

Page 87. § 66.

Mr. Scheele, at the end of this ſection, relates a very curious Experiment, namely, that *luna cornua* is eaſier reduced by the violet ray than by any other: this ſeems to favour the conjecture I had advanced in my remark on the 60th ſection, for the violet ray is the moſt refrangible of any therefore the more of it remains, and incorporates with bodies, that is, in this caſe, with the marine acid, which therefore ſooner parts with its phlogiſton to the calx of ſilver.

Page 91. § 67.

The earlieſt and moſt curious Experiments that have been made on this head, are thoſe of the celebrated Dr. Franklin, and the ſubject has been ſince ably proſecuted by Mr. Cavallo.

Page 103. § 72.

The principal characters of phlogiſton ſeem to me to be 1ſt. its ſtrong attraction to elementary Air, mineral acids, and metallic earths, together with the properties that reſult to theſe ſubſtances from their union with it.

2dly, Its repulſion with regard to elementary Fire, and Water.

3dly, The heat which always ariſes from its union with Air, or with any other ſubſtance whoſe capacity to contain Fire is greater than that of the body from which the phlogiſton was transferred.

4thly, Its properties in its pureſt ſtate; which I take to be that of inflammable Air from metals, well waſhed, though it cannot be ſaid to be quite pure, as it, even after waſhing, contains ſome metallic earth in ſolution, by reaſon of the great affinity or attraction they have to each other.

Page 105. § 73.

That metals may conſiſt of an acid and phlogiſton is not improbable, beſides the arſenical acid here mentioned; we may add, that the calxes of antimony, when well dephlogiſticated, are in ſome meaſure ſoluble in water; that even quickſilver imparts ſomething to it, that iron and copper ſeem to contain an acid, to ſay nothing of ſome other of the imperfect metallic ſubſtances; but earths, as far as is yet known, ſeem rather to be of an abſorbent nature.

Though the ſparry acid be little known, yet it may certainly be ſaid to contain phlogiſton;

giſton; for powdered flints make nitre deflagrate. *Home on Agriculture*, p. 30.

Page 107. § 74.

It is probable, that beſides the aërial acid, different oils eſſentially require different acids; the acid of ſugar is the baſis of ſome; and in animal oils, Mr. Crell has lately diſcovered a peculiar acid. Many of the vegetable acids are alſo known to poſſeſs properties eſſentially diſtinct from each other.

Page 109. § 75.

By *Fire* the generality of mankind underſtand the cauſe of heat, and by *heat*, they underſtand the ſenſation ſo called, or that diſpoſition of bodies that tends to excite that ſenſation in a greater or leſſer degree, whether attended with the ſenſation of light or not. But philoſophers finding that the bounds of ſenſation were very narrow, and its degrees very hard to be meaſured, beſtowed the ſame name on the *cauſe* of a much more unlimited effect, namely, that of *rarefaction*; finding that the ſame diſpoſition, which tended in *ſome* degrees of its intenſity to excite *heat*, tended in *all*, to excite rarefaction. Touching the nature of this cauſe they have been very much divided; ſome, as the Engliſh philoſophers in general, making it conſiſt in a tumultuous or vibratory motion of the proper and peculiar parts of the heated body; to which opinion, I find the celebrated Mr. Macquer has lately acceded; and it has been ingeniouſly ſupported by Mr. Lomonſow, in the Memoirs of Peterſburgh; others, among whom the great Boerhaave is the moſt eminent, have ſuppoſed it to be a peculiar element, diſtinct from all other ſubſtances except light, which is only a modification of it. The many curious and important diſcoveries of Dr. Black, and the ſtill later of Dr. Crawford, reflect the higheſt probability on this opinion. An excellent ſummary of theſe diſcoveries, with ſeveral ingenious applications of his own, has lately been publiſhed in French by Mr. Magellan, who is highly deſerving of the public gratitude for his extraordinary attention to the diffuſion of uſeful knowledge.

Mr. Wilſon has proved, in an elaborate tract, that all phoſphori ſhine by reaſon of a ſlow combuſtion; and Mr. Fontana has found that the Bononian phoſphorus in particular, diminiſhes Air.

Page 111. § 76.

Having refuted the principles of our author in the preceding ſections, I ſhall not pretend to rectify his miſtakes in the explanation of the phænomena, which would require a complete treatiſe on the ſubject, and more knowledge than I poſſeſs. In general, it may be remarked, that a very ſlight alteration will make many of his explanations conſiſtent with truth; for he aſſerts, that phlogiſton and pure Air compoſe heat; and in truth heat reſults from their union; becauſe, as Dr. Crawford has proved, elementary Air contains more Fire than any known ſubſtance, but by uniting with phlogiſton, its capacity for containing it is diminiſhed, and conſequently this Fire is let out, becomes redundant, and cauſes ſenſible heat.

Page 127. § 81.

The inflammation of pyrophori has been differently accounted for. Homberg, the original inventor, aſcribed the heat to the attraction of water by the earth of alum, which he had employed in making his pyrophorus; but this hypotheſis Mr. Suvigny has endeavoured to refute, by denying that aluminous earth heats with water; and he would have this heat proceed from the attraction of the vitriolic acid to water. On the other hand, Mr. Prouſt has demonſtrated the impoſſibility of this effect; as the vitriolic acid cannot be ſuppoſed in a diſengaged ſtate, while the earth of allum is preſent to which it has a ſtrong attraction; and Mr. Bewly ſtill better, by ſhewing that a pyrophorus may be made without any vitriolic acid; and, upon the whole, his hypotheſis ſeems to me moſt probable, namely, that charcoal, and a fixed alkali, or charcoal and an argillaceous baſis are alone neceſſary for the production of a pyrophorus. Charcoal is always neceſſary; the alkali is only ſo where the earth is not employed. The alkali, or earth, attracts an exceſs of phlogiſton; this exceſs is taken up by the atmoſpheric Air as ſoon as it is expoſed to it; the rapid ſolution of it in the Air produces the inflammation; this ſolution is accelerated by whatever haſtens the ſeparation of the phlogiſton from the coal or earth; and hence it is promoted by moiſture or heat.

The heat excited by the mixture of oil of vitriol and water, proceeds from the different quantities of Fire naturally contained in theſe ſubſtances before mixture, though of the ſame temperature; the quantity in oil of vitriol when concentrated to a certain degree and dephlogiſticated, I have found to be to that of water nearly as ſeven to ten; when therefore it combines with a certain proportion of water it expels from this latter a certain quantity of Fire, which then becomes redundant, and cauſes ſenſible heat.

The Experiments relating to the deſiccation of Air by quick-lime, and the inflammation of pyrophorus in cloſe veſſels are new and curious.

Mr. Fontana has given ſome proof that neither fixed alkalies nor oil of vitriol abſorb moiſture from the Air in cloſe veſſels. See Rozier's Journal for Jan. 1779.

Page 137. § 82.

This ſection contains, beſides a more exact account of the nature of fulminating gold, and a theory which (with very little alteration) may be rendered accurate, a new and important diſcovery, namely, the decompoſition of the cauſtic volatile alkali. The reigning opinion concerning fulminating gold hath hitherto been, that it contained a nitro-ammoniacal ſalt, which is known to inflame and explode in a ſmall degree of heat; but this opinion has been clearly refuted by Mr. Bergman, for he proved, 1ſt, that a ſolution of gold in the marine or vitriolic acid, if precipitated by a volatile alkali, would produce fulminating

ing gold. 2dly, That fulminating gold boiled in diſtilled water does not loſe its fulminating power. 3dly, That fulminating gold explodes more powerfully after it is freed from its adhering ſalts by waſhing, than before. The Experiments of Mr. Scheele go to prove that the calx of gold has a greater affinity with volatile alkalies than acids have with either; this calx therefore retains a volatile alkali until the moment of its reduction; but then, through its ſtronger affinity to phlogiſton, it decompoſes the volatil alkali, by ſtripping it of its phlogiſton; a ſpecies of Air is then ſuddenly produced, which as to the properties diſcovered in it by Mr. Scheele, reſembles phlogiſticated Air; the rapidity of its production cauſes the exploſion as in gunpowder.

Pages 146, 147. § 83, 84.

I cannot determine the quantity of Air abſorbed in this Experiment, as it is uncertain whether by *quart* ſhould be underſtood that meaſure which the Swedes call alſo a quart, and which is equal to half of one of our wine pints, or that which they call *kanne*; which is the word uſed in the German copy, from which this tranſlation was made, and which exceeds our wine gallon. However, it is certain the diminution was very inconſiderable; yet Dr. Prieſtley hath found it one-ſixth or one-fifth. It is very probable that in this caſe Air penetrated through the bladder, or that ſome was produced by the bread and milk. The next Experiment alſo is exceeding inaccurate, as it is hardly poſſible that ſome Air ſhould not be introduced; however, it is remarkable that our author found ſome fixed Air, though but one-thirtieth, by means of lime-water, which he would in vain ſeek for, if it had not been generated by breathing.

Page 148. § 85.

In theſe Experiments, Mr. Scheele finds that flies and bees die after ſome days in confined Air. Dr. Prieſtley has found, on the other hand, that inſects can live in putrid Air; perhaps in Mr. Scheele's Experiment they died for want of nouriſhment, which confined animals often refuſe. However, the quantity of Air diminiſhed, is very remarkable, it probably proceeds from the motion of theſe inſects, which enable them to phlogiſticate it throughout; for Mr. Keir has found that only the Air contiguous to an animal is infected, and that a candle will burn in a more diſtant part of the ſame Air, and above the animal; which ſhews that fixed Air only is formed by reſpiration, and not phlogiſticated Air; for this latter, being ſpecifically lighter than common Air, ſhould be found over it.

Page 149. § 86.

That vegetation hath a power of meliorating common Air is very evident from the Experiments of Dr. Prieſtley, ſee Vol. IV. p. 300; but that there are circumſtances which impede its ſalutary action is no leſs certain, though they be not all as yet well known; according to Dr. In-

Dr. Ingenhouz, light and darkneſs have oppoſite effects in this caſe, the former promoting, the latter impeding this amelioration; however, the great quantity of aërial acid produced in Mr. Sheele's Experiment, ſeems to me to proceed partly from the fermentation of the *peas*, which always precedes their ſprouting, and partly from the phlogiſton emitted during the fermentation. The remainder quenched a candle, becauſe it was phlogiſticated Air.

Page 152. § 89.

The reciprocal effects of Air and blood on each other, have been happily aſcertained by Dr. Prieſtley, in the third volume of his Obſervations; he has there ſhewn that red blood is freer from phlogiſton than black blood. That the former dephlogiſticates in ſome meaſure nitrous, inflammable, phlogiſticated, and fixed Air, and then becomes black: that the latter phlogiſticates common and dephlogiſticated Air, and then becomes red, particularly when the latter of theſe Airs is uſed;

and hence inferred the true uſe of reſpiration to conſiſt in the dephlogiſtication of blood, which he proved that Air could effect, even through the membranes of the lungs, and through ſerum. A diſcovery of the greateſt importance to medicine as well as to the general ſcience of nature.

Page 155. § 92.

That vegetables will not thrive in dephlogiſticated Air has alſo been obſerved by Dr. Prieſtley; who even diſcovered that many of them emit dephlogiſticated Air in certain circumſtances, as an excrementitious ſubſtance, which diſcovery has been confirmed by Dr. Ingenhouz. Yet it is not improbable that in other circumſtances dephlogiſticated Air may be converted by them into fixed Air, as Mr. Scheele aſſerts.

Page 156. § 93.

The conſequences here drawn by Mr. Scheele are much too general. Vegetables evidently thrive better in phlogiſticated, than in pure Air. Dephlogiſticated Air cannot be looked upon as an acid united to phlogiſton, ſince it affords no marks of either, particularly the latter; it is much more probable that vegetables derive their phlogiſton from phlogiſticated Air, than from light or heat, as neither of theſe can be ſaid to contain it, as already ſhewn. Light certainly promotes the dephlogiſtication of Air, but it is probably by means of a double decompoſition; light uniting to the Air and increaſing its quantity of Fire, while the phlogiſton unites to vegetables, as explained in my remark on the 60th ſection.

That the lungs do change empyreal or dephlogiſticated Air into aërial acid, is very evident by breathing it into lime water, which is ſoon made turbid.

It is hard to ſay how inflammable Air could have been reſpired ſo often by Mr. Scheele without inconvenience, and how it has happened in this Experiment, that it loſt its inflammability; for not only Dr. Prieſtley, but all others who have experienced its effects on animals, have found it deleterious; and alſo, that after animals had died in it, it ſtill remained inflammable. Mr. Fontana has indeed ſhewn, that when the lungs are not entirely emptied of the Air they naturally contain, inflammable Air may be breathed for ſome time with impunity, but that it cannot, when the lungs are entirely emptied; yet Mr. Scheele ſays, that even in this manner he has reſpired it ten times.

The Experiments of Dr. Prieſtley, relative to filings of iron, water and ſulphur, expoſed to fixed Air, prove only that fixed Air was rendered immiſcible with water, but not that it was turned into common Air; it appears rather to have been in part converted into phlogiſticated Air, by the ſuperſaturation of the aërial acid with phlogiſton. See his Obſervations, Vol. I. p. 42, &c.

Mr. Scheele ſays, he failed in ameliorating inflammable and phlogiſticated Air, (which he calls foul Air) by agitation in water, but his failure evidently aroſe from his agitating theſe Airs in water, which had no communication with the atmoſphere;

mosphere; whereas Dr. Prieſtley always ſucceeded by attending to this circumſtance.

Page 163. § 94.

The reſult of the firſt Experiment here mentioned agrees with that which Dr. Prieſtley alſo found, Vol. I. p. 158. by which it clearly appears that water attracts the dephlogiſticated part of common Air preferably to the phlogiſticated part. The ſecond Experiment is rather contrary to his trials, for he found, that even inflammable Air is in ſome meaſure abſorbed by water, out of which its own Air was boiled, Vol. III. p. 267. The third Experiment is confirmed by Mr. Fontana. Dr. Prieſtley obſerved, that pump-water yielded dephlogiſticated Air, by mere expoſure to ſun-ſhine.

The manner in which Mr. Scheele diſcovers dephlogiſticated Air in water, is truly ingenious. The remainder of this ſection contains new and important truths, and happily explains why water diſtilled even in glaſs veſſels phlogiſtates the Air to which it is expoſed, as Dr. Prieſtley has remarked.

Page 168. § 95.

After what has been already ſaid of the nature of heat, it is unneceſſary to follow Mr. Scheele in the detail of the Experiments here mentioned; it is ſufficient to obſerve, that the pureſt water contains more of what he calls heat, than any of the ſubſtances he mentioned, as Dr. Crawford has proved at large; and ſurely without overturning all our ideas, it cannot be ſaid to contain more acid; but by a ſlight alteration in the expreſſion, all the phænomena here mentioned, may be eaſily explained conſiſtently with the ſyſtem of Dr. Black and Dr. Crawford.

Page 178. § 96.

The very reverſe of the maxim here laid down by Mr. Scheele, namely, that metals contain ſo much more heat, as they contain more phlogiſton, has ſince been demonſtrably proved by Dr. Crawford.

Dr. Prieſtley has extracted inflammable Air from metals in cloſe veſſels full of quickſilver, which directly overturns this part of Mr. Scheele's ſyſtem,

Page 186. § 97.

The Air treated of in this ſection was firſt diſcovered by Mr. Scheele, nor does it appear to have been as yet inveſtigated by any other philoſopher; it ſeems to be a ſolution of ſulphur in vitriolic or marine acid Air.

LETTER, &c.

LETTER, &c.

To RICHARD KIRWAN, *Eſq.*

DEAR SIR,

I THINK myſelf much obliged to you for the Notes which, at my requeſt, you have been ſo obliging as to write on this work of Mr. Scheele. I was very deſirous that a treatiſe of one of the firſt chemiſts in Europe, on a ſubject in which he has been a fellow-labourer with myſelf, ſhould be eaſily underſtood in this country; and as I was ſenſible that many things in it would want illuſtration, and probably ſome correction, a mere tranſlation, however well executed, would not be ſufficient. I am far from pretending to a complete knowledge of chemiſtry, but Dr. Forſter's tranſlation, and your Notes together, ſeem to have anſwered all my wiſhes; and all that I have to add, are a few obſervations on ſome points of difference between Mr. Scheele and myſelf, with illuſtrations from ſome of my late unpubliſhed Experiments.

Mr. Scheele always found that when he expoſed fixed Air to iron filings and brimſtone, it was almoſt wholly abſorbed by by that mixture; whereas, I had found, (Vol. I. p. 41.) that after this proceſs, a conſiderable proportion of the fixed Air was immiſcible with water, though not rendered fit for reſpiration, as he, by a miſtake of my meaning, ſays.

On this I would obſerve, that the *fact* was exactly as I repreſented it; but I now think that I was miſtaken with reſpect to the *cauſe*. In the trials which I then made with this mixture, whether in water, or in vacuo, no Air of any kind was produced; but I have ſince found, that by allowing it more time, inflammable Air is yielded by it; though when a conſiderable quantity of common Air is expoſed to it, it is ſimply phlogiſticated by the proceſs, and nothing inflammable is found in it.

In other proceſſes alſo, in which reſpirable Air is ſimply phlogiſticated, *I* have found that when it is made in quickſilver, or water, without any Air, a ſmall quantity of inflammable Air has been produced; ſo that when inflammable Air is generated very ſlowly, and has immediate acceſs to a ſufficient quantity of reſpirable Air, it is decompoſed as it is formed, the phlogiſton incorporating with reſpirable Air; whereas, if a large quantity of inflammable Air be mixed with common Air, they will continue together a very long time, without much affecting each other.

I was led to theſe obſervations, by inquiring into the cauſe of the ſeeming converſion of nitrous Air into inflammable Air, mentioned in the laſt volume of my Experiments. For uſing leſs and leſs nitrous Air, and at laſt none at all, I found that inflammable Air was continually, though ſlowly, produced from the iron filings and brimſtone; and this Air being in time more in quantity than the nitrous Air, (which itſelf becomes mere phlogiſticated Air by long ſtanding in water) the reſult of the whole proceſs was a pretty ſtrong inflammable Air.

You have yourſelf very well obſerved the difference between the methods in which

Mr. Scheele and myſelf agitated both phlogiſticated and inflammable Air in water, in conſequence of which we had different reſults.

The fact of the *purification of reſpirable Air by vegetation*, which Mr. Scheele denies, has been ſo fully proved, that I ſhall only inform you and our readers of what I had obſerved before the publication of Dr. Ingenhouz's book on that ſubject. At the time of the publication of my laſt volume, I had obſerved that a *green matter*, which I then ſuppoſed to have been depoſited by the water, yielded a great quantity of very pure Air when expoſed to *light*, but that mere *warmth* had no ſuch effect upon it. I at firſt ſuſpected this green matter to be a plant, but not being able to diſcover any thing like the *form* of a plant by a microſcope, I continued to call it ſimply green matter. Several of my friends, however, who were better ſkilled in botany, never entertained any doubt of its being a plant; and preſently after the publication of that volume, I had myſelf the cleareſt proof, that though I could not diſcover the lineaments of a plant, it *muſt* neceſſarily be a vegetable ſubſtance. Immediately upon this, I tried other plants that grow wholly in water, and found that all of them, without exception, produced the ſame effect, yielding, when placed in the light, and eſpecially in that of the ſun, a certain quantity of the pureſt Air. This proceſs, however, as in that of the green matter, always had its *limits*, never exceeding one tenth, or at the moſt one eighth of the capacity of the veſſel in which it was made.

The quantity of Air thus produced, bearing a certain proportion to the quantity of water, I was ſatisfied it muſt be the water, or ſomething contained in the water, that furniſhed the materials for this pure Air; and this I aſcertained by changing the water after the production of Air had ceaſed. For the moment that the ſame plants, which now yielded no Air at all, even in the ſun, were put into a quantity of *freſh water* of the ſame kind, they immediately yielded Air as briſkly and in as great a quantity as before; but after this, the production of Air ſtopped 'till the water was changed again.

That it was the *Air* contained in the water, and not the *water itſelf* that furniſhed the materials for this pure Air, I found by examining the Air contained in the water before and after this proceſs. For expelling Air by heat from the water which had not been uſed in this manner, I found it to be either of the ſtandard of common Air, or ſomething below it; but when this water had been uſed in this manner, that is, when it had been expoſed to the action of the plants and of the ſun, it yielded the pureſt Air; this being ſtill retained in the water after a conſiderable quantity more than could be contained in it had been diſengaged from it, and lodged on its ſurface. This pure Air alſo contained in the water was in much greater *quantity* than the impure Air contained in it before. For as pure Air is diminiſhed whenever it is phlogiſticated, ſo, on the contrary, the quantity is encreaſed when it is dephlogiſticated.

The action of theſe plants therefore being to dephlogiſticate the Air contained in water in which they grow, I immediately concluded, that *all plants*, growing in water, or out of water, muſt purify the Air to which they have acceſs, whenever they are placed in a ſufficiently ſtrong light, and therefore that there could be no room to entertain any doubt with reſpect to my firſt obſervations on this ſubject, though at that time I was ignorant how much *light* had to do in the buſineſs.

I alſo found that ſome plants growing in Air, produced this effect in a ſmall degree when they were put into water, though this not being their natural element, they could not bear it long; but I did not ſuſpect that *leaves alone* would retain ſo much life as to produce this effect, when ſeparated from the plants, until I ſaw Dr. Ingenhouz's book, which abundantly proves it, and which I had a great pleaſure in peruſing, notwithſtanding ſome miſtakes he has been led into, and which I know I ſhall give him pleaſure in rectifying in my next publication on this ſubject. Some of them I mentioned to him in perſon afterwards.

You have, I perceive, adopted the opinion, that phlogiſton converts atmoſpherical Air into fixed Air, in which I cannot ſay that I am at preſent diſpoſed to agree with you. I have, indeed, been inclined to think that fixed Air may be a factitious ſubſtance, and have deſcribed ſome proceſſes in which I thought I procured it in conſiderable quantities from materials which had not been ſuppoſed to contain it; and thence I was led to ſuſpect that the appearance of it, even in the phlogiſtication of common Air *might* be occaſioned by a real production of it, though I had no conjecture concerning the immediate cauſe of that production. But, upon the whole, I am inclined to think that all the fixed Air that is diſcovered in this caſe, is nothing more than that which is naturally contained in, and incorporated with, the atmoſpherical Air; and therefore that it is merely *precipitated* by the phlogiſtication of that Air.

I think that if phlogiſton be capable of converting any *part* of a maſs of Air into fixed Air, it might make the ſame change in the *whole* of it, which is never the caſe; and no more fixed Air is produced in phlogiſtic proceſſes than the common Air may be ſuppoſed to contain. That it does contain *ſome* ſeems to be evident from the precipitation of lime in lime-water, ſimply expoſed to the common Air. I was engaged in a courſe of Experiments, which I think calculated to decide this queſtion, when my late tedious illneſs, and the change of my ſituation, with the conſequent interruption of all my Experiments for almoſt the whole of this ſummer, prevented the completion of them.

Mr. Scheele explains the phænomenon of *fulminating gold*, by ſuppoſing a generation of Air, to which he gives the properties of phlogiſticated Air. Now, whenever I have thought upon the ſubject, ſince the diſcovery of *alkaline Air*, I have had no doubt but that the exploſion of that ſubſtance muſt be occaſioned by the ſuddenly ſetting looſe a quantity of that kind of Air; and becauſe a much greater quantity of this Air may be contained in many ſubſtances than of fixed Air; and alſo becauſe the whole of it may perhaps be reſtored to its elaſtic ſtate more nearly at the ſame time, this exploſion is greater than that which is made by the *pulvis fulminans*, or any other compoſition into which merely fixed Air enters. The ſame ſolution has likewiſe occurred to other perſons ſince the diſcovery of alkaline Air; and I even fully intended to have mentioned this ſolution in two of my laſt publications on the ſubject of Air, but it eſcaped my memory at the time.

Your partiality to me is too apparent in the courſe of your Notes; however, I am truly ſenſible of the value of that *friendſhip* from which the partiality proceeds; and am, with much reſpect,

Dear Sir,

Your obliged humble ſervant,

J. PRIESTLEY.

London, Aug. 1780.

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